

# Zetetic scholar

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# Zetetic scholar

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SCIENTIFIC REVIEW OF CLAIMS OF  
ANOMALIES AND THE PARANORMAL



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# EDITORIAL

My apology to all -- especially the authors of the articles in this issue -- for your patience and encouragement in getting out this long overdue issue of ZS. Most of you know ZS is a labor of love and pretty much a one man operation. The unconscionable delay was produced out of a strange combination of my procrastination and overload. Unlike our current politicians, I accept the blame along with the responsibility. Thank you for your indulgence and your many kind letters urging me to continue this project. I can only hope that the contents of this double-sized issue might in part compensate for the delay.

Henceforth, to avoid future "catch-up" problems, ZS will officially be on an irregular schedule although I still hope to get at least two issues out per year. A subscription will continue to consist of two numbers. The next issue (#14) is already largely in my computer, so it should be out on schedule (within six months). Meanwhile, a number of CSAR projects are now being developed, including publication of the CSAR newsletter. Again, my thanks for your past support and your patience.

\*\*\*\*\*

Since publication of the last issue, death came to a startling number of the major contributors to ZS: George Abell, Milbourne Christopher, Eric J. Dingwall, J. Allen Hynek, Walter Gibson, Richard Kammann (all Senior Consultants to CSAR) and Piet Hein Hoebens (who frequently contributed to the pages of ZS, was --as he preferred it-- my "unofficial associate editor," and had a profound influence on my own views about anomalies). They all are and will be greatly missed.

# ON PSEUDO-SKEPTICISM

Over the years, I have decried the misuse of the term "skeptic" when used to refer to all critics of anomaly claims. Alas, the label has been thus misapplied by both proponents and critics of the paranormal. Sometimes users of the term have distinguishing between co-called "soft" versus "hard" skeptics, and I in part revived the term "zetetic," because of the term's misuse; but I now think the problems created go beyond mere terminology, and matters need to be set right. Since "skepticism" properly refers to doubt rather than denial -- nonbelief rather than belief -- critics who take the negative rather than an agnostic position but call still themselves "skeptics" are actually pseudo-skeptics and have, I believed, gained a false advantage by usurping that label.

In science, the burden of proof falls upon the claimant; and the more extraordinary a claim, the heavier is the burden of proof demanded. The true skeptic takes an agnostic position, one that says the claim is not proved rather than disproved. He asserts that the claimant has not borne the burden of proof and that science must continue to build its cognitive map of reality without incorporating the extraordinary claim as a new "fact." Since the true skeptic does not assert a claim, he has no burden to prove anything. He just goes on using the established theories of "conventional science" as usual. But if a

critic asserts that there is evidence for disproof, that he has a negative hypothesis (e.g., the conjecture that a seeming psi result was actually due to an artifact), he is making a claim and therefore also has to bear a burden of proof. Sometimes, such negative claims by critics are also quite extraordinary (e.g., that a UFO was actually a giant plasma or that someone in a psi experiment was cued via an abnormal ability to hear a high pitch others with normal ears would fail to notice), in which case the negative claimant also may have to bear a heavier burden of proof than might normally be expected.

Critics who assert negative claims but who mistakenly call themselves "skeptics" often act as though they (as would be appropriate only for the agnostic or true skeptic) have no burden of proof placed on them at all. A result of this is that many critics seem to feel it is only necessary to present a case for their counter-claims based upon plausibility rather than empirical evidence. Thus, if a subject in a psi experiment can be shown to have had an opportunity to cheat, many critics seem to assume not merely that he probably did cheat but that he must have, regardless of what may be the complete absence of evidence that he did so cheat (and sometimes ignoring evidence of the subject's past reputation for honesty). Similarly, improper randomization procedures are sometimes assumed to be the cause of a subject's high psi scores even though all that has been established is the possibility of such an artifact having been the real cause. Of course, the evidential weight of the experiment is greatly reduced when we discover an opening in the design that would allow an artifact to confound the results. Discovering an opportunity for error should make such experiments less evidential and usually unconvincing; it usually disproves the claim that the experiment was "air tight" against error, but it does not disprove the anomaly claim. Showing evidence is unconvincing is not grounds for completely dismissing it. If a critic asserts that the result was due to artifact X, that critic then has the burden of proof to demonstrate that artifact X can and probably did produce such results under such circumstances.. Admittedly, in some cases the appeal to mere plausibility that an artifact produced the result may be so great that nearly all would accept the argument (e.g., when we learn that someone known to have cheated in the past had an opportunity to cheat in this instance, we might reasonably conclude he probably cheated this time, too); but in far too many instances, the critic who makes a merely plausible argument for an artifact closes the door on future research when proper science demands that his hypothesis of an artifact should also be tested. Alas, most critics seem happy to sit in their armchairs producing post hoc counterexplanations. Whichever side end up with the true story, science best progresses through laboratory investigations.

On the other hand, proponents of an anomaly claim who recognize the above fallacy may go too far in the other direction. Some argue, like Lombroso when he defended the mediumship of Palladino, that the presence of wigs does not deny the existence of real hair. All of us must remember science can tell us what is empirically unlikely but not what is empirically impossible. Evidence in science is always a matter of degree and is seldom if ever absolutely conclusive. Some proponents of anomaly claims, like some critics, seem unwilling to consider evidence in probabilistic terms, clinging to any slim loose end as though the critic must disprove all evidence ever put forward for a particular claim. Both critics and proponents need to learn to think of adjudication in science as more like that found in the law courts, imperfect and with varying degrees of proof and evidence. Absolute truth, like absolute justice, is seldom obtainable. We can only do our best to approximate them.

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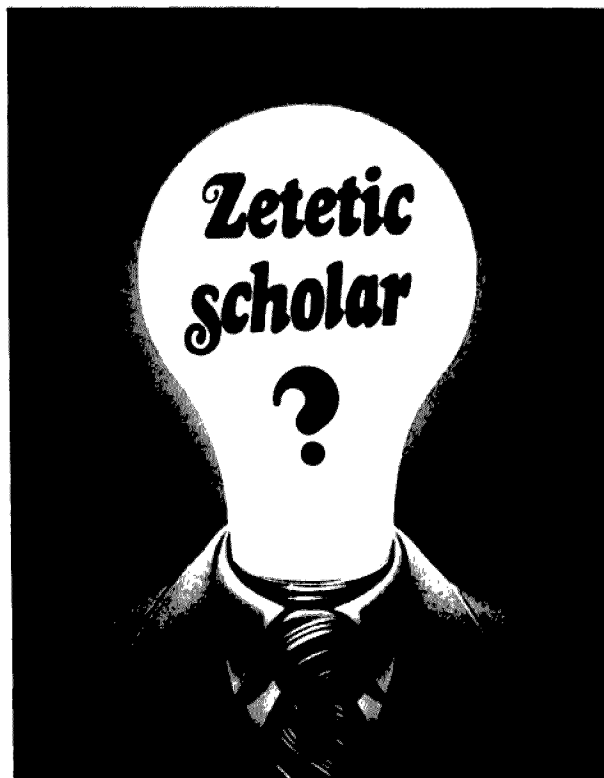
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We think *Zetetic Scholar* is special. And we think our subscribers are special, too. *ZS* was never intended for everyone. Our goal is to seek quality rather than quantity, both in our articles and our readers. *ZS* is published with an intentionally limited circulation (600). We know that most people are looking for answers, for certainties, while the main job of *ZS* has always first been to seek out the right questions. We find most people seem to be dogmatic when it comes to claims about anomalies and matters paranormal. "True Believers" and "True Disbelievers" are common. But we are looking for readers who have not yet made up their minds, people who can be tolerant of "loose ends" and don't want to rush to judgement -- to either one side or the other. So, if you are sympathetic to our aim of publishing fair-minded and balanced dialogues about scientific anomaly claims, we hope you will support *ZS* as both a subscriber and a contributor. And we hope you will tell others about us.

For those interested in greater involvement with *ZS* and *CSAR*, details about can be found at the end of this issue.

A subscription to *ZS* purchases two issues (numbers). Subscriptions in the U.S. and Canada are \$15 (U.S.), and foreign subscriptions are \$20 (surface mail) or \$30 (airmail). No foreign currency or checks on foreign banks, please, since service charges are prohibitive.

## ZETETIC RUMINATIONS ON SKEPTICISM AND ANOMALIES IN SCIENCE

Marcello Truzzi

### I. On Zeteticism

When I first adopted the term *zetetic* to find an original name for a private newsletter dealing with scientific perspectives on anomalies, I thought that archaic term would likely avoid controversy. That was eleven years ago and mainly was the result of my looking for a term to replace the original title, *Explorations*, which some of my readers objected had already been adopted by other publications. *Zetetic's* dictionary meaning was simply "skeptical inquirer," and the last modern use of the term had been by the Flat Earth Society in the 19th century. But what started out to be an uncontroversial label soon emerged in my mind as perhaps the most controversial label of all. Through serendipity --some might argue synchronicity-- I had stumbled on something now very significant to me, for I found upon researching the term that it both well describes and shapes my current orientation towards anomalies.

The term *zetetic* (both a noun and an adjective) was first applied to the followers of the Greek skeptical philosopher Pyrrho of Ellis (ca. 365-275 B.C.). Pyrrho urged suspension of judgement about facts, that we should "be without beliefs, disinclined to take a stand one way or another, and steadfast in this attitude" (Stough, 1969: 26 n.23). But, as Richard H. Popkin has noted, Pyrrhonism should not be confused with Academic Skepticism that stemmed from Socrates' statement that "All I know is that I know nothing," the view that no knowledge can be certain. The *zetetics* took a more moderate position. As Popkin points out in his history of skepticism:

The Pyrrhonists considered that both the Dogmatists and the Academics asserted too much, one group saying "Something can be known," and the other saying that "Nothing can be known." Instead, the Pyrrhonians proposed to suspend judgement on all questions on which there seemed to be conflicting evidence, including the question whether or not something can be known.... The Pyrrhonian sceptics tried to avoid committing themselves on any and all questions, even as to whether their arguments were sound. Scepticism for them was an ability, or mental attitude, for opposing evidence both pro and con on any question about what was non-evident, so that one would suspend judgement on the question.... Scepticism was a cure for the disease called Dogmatism or rashness. But unlike Academic scepticism, which came to a negative conclusion from its doubts, Pyrrhonian scepticism made no such assertion, merely saying that scepticism is a purge that eliminates everything including itself. The Pyrrhonist, then, lives undogmatically, following his natural inclinations, the appearances he is aware of, and the laws and customs of his society, without ever committing himself to any judgement about them. [Popkin, 1979: xv]

Like a proper *zetetic*, I remain uncertain about the ultimate correctness of

this perspective, but as a working scientist, I find its practical function in avoiding dogmatism is most valuable. That is, this orientation is **heuristic** in that it emphasizes questions rather than answers. It fits the aims of what Gunter (1980) has called "Story Book" science (if not that actually practiced) while it avoids mistaking the goals of scientific method with science's current substantive content. But perhaps most important of all, I find this form of skepticism congruent with the fallibilism of modern philosophies of science and with the injunction of Charles Sanders Pierce that the foremost duty of the philosopher-scientist is to do nothing that might block inquiry (Peirce, 1966: 56).

Regretfully, the term "skeptic" today is being used by many who adopt that label for themselves in a misleading way. To many, it is falsely equated with the term "rationalist." The dictionary meaning of the term indicates that a skeptic is one who raises doubts. Thus the word is meant to reflect **nonbelief** rather than **disbelief**. But when we look at those who trumpet that they are skeptics towards claims of anomalies, we find disbelievers and debunkers rather than those who express uncertainty or doubt. The public "skeptics" of today present us with answers rather than questions. As philosopher W.V. Quine (himself, ironically, one among such modern public "skeptics") neatly made the distinction:

It is important to distinguish between disbelief and nonbelief-- between believing a sentence is false and merely not believing it true. Disbelief is a case of belief; to believe a sentence false is to believe the negation of the sentence true. We disbelieve that there are ghosts; we believe that there are none. Nonbelief is a state of suspended judgement; neither believing the sentence true nor believing it false. [Quine and Ullian, 1978: 12]

Of course, none of this is to suggest that disbelief is always in error or that there is not bunk that needs to be debunked. I only point out that disbelief should not be confused with skepticism and nonbelief. This confusion is far from a new problem, and James H. Hyslop --who would probably disagree with Quine about ghosts-- noted the confusion in a 1909 article in the Journal of the American Society for Psychical Research when he wrote:

The average man today thinks he is a sceptic because he does not believe in a given allegation. The fact is that scepticism is not unbelief in the sense of denial nor in the sense of being opposed to a given belief, but it is critical ignorance. Few men show this characteristic. They are too much ashamed of denying what they do not know something about. The public has gotten into the attitude of mind which it likes to call scepticism, but which is nothing more or less than dogmatism hiding under false colors. It thinks that belief is the only thing that can be biased and does not dream that denial can be biased, and in fact that the bias of denial is not only less justifiable but far worse than the bias of belief. It has no basis upon which to rest at all except belief. But people have come to think that denial or doubt is the mark of intelligence, when in fact true scepticism is much nearer being a mark of ignorance. True scepticism means that we do not know, not that such a thing is not

true. To know that a thing is not true is knowledge, not doubt, and hence is subject to bias. It is all the worse when it parades itself as a trustworthy student of truth and in fact is only trying to deny it. The average mind assumes that belief disqualifies a man from studying a problem and that the only person who can investigate it is the man who does not believe anything about it. If the doubter has no opinions and is not biased by preconceptions of his own, and if he does not have an interest in an opposing theory, it is true that he may be better qualified than the believer to investigate, but the majority of those who parade as sceptics in the matter usually have some theory of their own to sustain against that which they claim not to believe, and hence are as much biased as the despised believer.... [O]pen-mindedness is the only scepticism that can claim immunity from prejudice. [Hyslop, 1909: 29-30]

Though Hyslop called our attention to this confusion 75 years ago, most anomaly researchers, instead of observing this distinction between nonbelief and disbelief, seem to have accepted the critics' own definition of themselves as skeptics and constantly misrefer to them as such. This has resulted in a most artificial polarization between believers and disbelievers, misrepresents both the options and the reality of the opinions held, and makes us zetetics --the true skeptics --either invisible or forced to choose sides or be thought of as "the enemy" by both sides.

In his now classic discussion of the normative structure of science, Robert K. Merton included organized skepticism along with universalism, communism and disinterestedness among the institutional imperatives of science (Merton, 1973: 270). He referred to this as the "temporary suspension of judgement and the detached scrutiny of beliefs in terms of empirical and logical criteria," and then pointed out that this practice "may come into conflict with other attitudes toward the same data which have been crystallized and often ritualized by other institutions" (Merton, 1973: 277). I would suggest to you that this conflict also occurs between one part of the scientific community and another. As our scientific institutions have developed, this becomes an internal as well as an external problem. And as these institutions have become integrated into other institutions, vested interests and non-scientific concerns (such as the control of economic resources) develop. And I suggest that as science grows into so called "Big Science," the norm of organized skepticism begins to conflict frequently with the norm of disinterestedness. This can lead to attempts by defenders of the majority or "orthodox" viewpoint to attempt to merely discredit rather than disprove competitive minority views (especially maverick claims), and this results in what Ray Hyman (1980) has pointed out is a form of "pathological science."

As Thomas S. Kuhn (1977) has termed it, there is an "essential tension" within science since it must on the one hand preserve its accumulated knowledge by acting cautiously and conservatively while on the other hand remain an open system ready to take in new and potentially revolutionary data and concepts. This balance is maintained through a number of methodological prescriptions which make it difficult but not impossible for the claimant of an anomaly to obtain acceptance of the claim. First, science places the burden of proof on the claimant. Second, the proof for a claim must in some sense be commensurate with the character of the claim. Thus, an extraordinary claim

requires "extraordinary" (meaning stronger than usual) proof. This latter prescription seems related to the rule of parsimony in science that states that the simpler adequate explanation is the one to accept.

Now I would call your attention to the fact that these rather conservative rules for evidence of extraordinary claims mean that a claim that is inadequately supported results in a simple nonacceptance of the claim. Evidence is, then, a matter of degree, and not having enough results in a claimant's not satisfying the burden of proof. It does not mean disconfirmation of the claim. The proof is insubstantial, and the claim is unaccepted rather than refuted. The claimant is, in effect, told either to give up or go back to find stronger evidence and arguments for a possible later day in the court of science. As a practical matter, an unproved fact is a non-fact. Science assumes the negative about unproved claims; it gives such claims low priority and low probability and ignores them. Since science is essentially descriptive (creating explanations through abstracted generalizations made about ascertained facts), it is not prescriptive (Taylor, 1963: 342). Science can speak of the highly improbable; it can not properly speak of the impossible. But as a practical matter, the highly improbable is treated as though it were impossible. Working on a perpetual motion machine is almost certainly a waste of time, but once we deem it absolutely a waste of time, we close the door on such research and violate the equilibrium of the "essential tension" and disobey Peirce's injunction by blocking inquiry. The scientist who works on a perpetual motion machine may be playing the longest shot of all, and he may be conducting stupid science, but it is not necessarily false or pseudoscience.

Since the scientific community sets its priorities based upon the notions of probable pay-offs and importance of problems, the claimant of an unproven anomaly is playing a long shot and is unlikely to obtain scarce resources. And the unproven idea or anomaly may be, and usually is, ignored or treated as would be a false idea. So, I would suggest that this actually allows for greater tolerance than is often accorded claims of the extraordinary. If we really have faith in the scientific rules and the self-correcting mechanisms of science, we have little to fear from unusual low-probability or even crackpot ideas. We don't have to disprove an idea to discount it; we merely need to show its failure to bear its burden of proof. Further, I would suggest that concern with discrediting rather than disproving claims actually shows a lack of confidence in the normal adjudication system of science. Just as a proper system of justice does not require vigilantes, so does a proper scientific adjudication process not need any extra-scientific inquisition to protect us from alleged pseudoscience and so-called irrationality.

Organized skepticism in science is a two-edged sword. It allows us to question orthodoxy as well as unorthodoxy. Its proper functioning depends upon our tolerance and our ability to live with a certain degree of uncertainty and dissonance in our ranks. Robert K. Merton has spoken of the sociological ambivalence built into social roles (Merton, 1976). The conflict produced by science's need for both conservation and openness--what Kuhn speaks of as creating the "essential tension" proper to science--demands the organized skepticism (so integral to science and which produces its self-correcting pattern of change that most of us think of as progress) which contributes greatly to that ambivalence. The tension has also been characterized as a struggle to avoid both kinds of what statisticians refer to as Type I and Type II errors. We don't wish to conclude something anomalous is out there when it is not, but we also do not want to overlook some rare anomaly, some weak

signal amidst much noise (Truzzi, 1979a and b).

Most of my fellow critics of anomalies have characterized their own harsher and debunking-oriented approach as representing a "hard-line" skepticism while my zeteticism is described as a "softer-line" variety. I think this misrepresents the reality of our differences. The public is indeed often misled, and we need to have critical views presented to them to balance those of the proponents. Bunk needs to be debunked; fraud needs to be exposed; error needs to be corrected; and real pseudoscience--and it does exist--needs to be shown up for what it is. But fraud, error and even pseudoscience have all existed within ordinary science, that is within legitimate, orthodox science. And much that gets labelled "pseudoscience" actually is not, for much so-called pseudoscience consists of scientific research programs (what I have called protosciences) being applied to investigate unorthodox or controversial matters (Truzzi, 1972 and 1977). I do not agree with those who equate science with a body of content; for me, science centrally remains a method. A science that studies unicorns may (and properly done, should most likely) find that no unicorns exist; but the search for unicorns can be conducted in scientific fashion. Critics forget that a person does not have to believe in ESP to be a parapsychologist or in UFOs to be a ufologist. Part of what science does is to determine whether or not variables exist; so to that degree, a science can investigate "nothing."

The reason that the zetetic may appear to take a softer critical line may be the result not so much of a less critical view of the extraordinary but of a more critical view of the ordinary with which it is contrasted. Those who leap to call parapsychology a pseudoscience might do well to look more closely at the social sciences in general. Those who laugh at the implausibility of a possible plesiosaur in Loch Ness should take a close look at the arguments and evidence put forward for the Big Bang or Black Holes. Those who think it unreasonable to investigate reports of unidentified flying objects might do well to look carefully at the arguments and evidence of those who promote current attempts at contacting extraterrestrial intelligence allegedly present in other solar systems. Those who complain about the unscientific status of psychic counselors should be willing to examine the scientific status of orthodox psychotherapy and make truly scientific comparisons. Those who sneer at phoney prophets in our midst might also do well to look at the prognosticators in economics and sociology who hold official positions as "scientific forecasters." Those who concern themselves about newspaper horoscopes and their influence might do well to look at what the "real" so-called helping professions are doing. The scientist who claims to be a true skeptic, a zetetic, is willing to investigate empirically the claims of the American Medical Association as well as those of the faith healer; and, more important, he should be willing to compare the empirical results for both before defending one and condemning the other.

The skeptical posture of the scientist, I suggest, is one which T.H. Huxley termed agnosticism. Today, that term usually is thought to refer only to an orientation towards the question of the existence of a deity. But when Huxley first put the term forward, he meant it far more generally. And --note a moral for today-- the term was meant to avoid the dogmas of both the Rationalists and the Religionists; in the Metaphysical Society of 1889. Huxley wrote:

Agnosticism in fact, is not a creed, but a method, the essence of which lies in the rigorous application of a

simple principle: That principle is of great antiquity... it is the fundamental axiom of modern science. Positively the principle may be expressed: In matters of the intellect, follow your reason as far as it will take you, without regard to any other consideration. And negatively, in matters of the intellect, do not pretend that conclusions are certain which are not demonstrated or demonstrable. That I take to be the agnostic faith, which if a man keep whole and undefiled, he shall not be ashamed to look the universe in the face, whatever the future may have in store for him. [Huxley, 1889]

Huxley recognized that such tolerance was an ideal and not something he always was able to put into practice, for he went on to say:

The results of the working out of the agnostic principle will vary according to individual knowledge and capacity, and according to the general condition of science. That which is unproven today may be proved by the help of new discoveries tomorrow. The only negative fixed points will be those negations which flow from the demonstrable limitation of our faculties. And the only obligation accepted is to have the mind always open to conviction. Agnostics who never fail in carrying out their principles are, I am afraid, as rare as other people of whom the same consistency can be truthfully predicated. But if you were to meet such a phoenix and tell him that you had discovered that two and two makes five, he would patiently ask you to state your reasons for that conviction, and express his readiness to agree with you if he found them satisfactory. The apostolic injunction to "suffer fools gladly" should be the rule of life of a true agnostic. I am deeply conscious of how far I myself fall short of this ideal, but it is my personal conception of what agnostics should be. [Huxley, 1889]

Like Huxley, I think this a good ideal to uphold for the scientist, but it remains difficult to place it into practice. I therefore modify it somewhat. I contend that every scientist has a right to his/her own priorities. So, when a presumed crackpot approaches you with a wild idea, I do not think you are duty bound to listen and evaluate it. You have the full right to simply say you are too busy or to ignore such claimants. The claimant does not have a right to your time. But I contend that if a hearing is granted, it should be a fair and open-minded hearing based on consideration of the arguments and evidence. And if the arguments and evidence have some merits, even if they are inadequate to bear the burden of proof, we should be willing to admit such merits while still failing to accept (rather than necessarily denying) the claim. We must remember that evidence is always a matter of degree, and usually not overwhelming. And we must be willing to admit that even if we reject much evidence and argument, that there are likely to be "loose ends" that often can not be easily explained away. Most of all, we must remember that the goal of science is to **explain** rather than **explain away** phenomena. Skepticism should seek to be **constructive**, to advance science, rather than purely destructive and perhaps thereby to block inquiry.

Having now considered some of the characteristics of skepticism in sci-

ence, let me turn now to the subject of anomalies.

## II. On Anomalies

The dictionary definition of an anomaly as a "deviation from the common rule" or "something that deviates in excess of normal variation" is rather vague and somewhat misleading in that this definition also fits "irregularity" or "abnormality." As commonly used in the literature concerned with anomalies, the term emphasizes that an anomaly is something **unexplained**. An anomaly is something that is not covered by our current generalizations about how the world operates. It is something strange and unaccounted for. As we look at the term's use in the literature, it becomes rather multi-dimensional and is more like a constructed type or a fuzzy set (Westrum and Truzzi, 1978: 70). In its pure state, an anomaly might be something that (1) actually occurs (that is, something both perceived and validated), (2) is not explained by some accepted scientific theory, (3) is perceived to be something which is in need of explanation, and (4) contradicts what we might expect from applying our accepted scientific models. In practice, an anomaly is often "impure" in terms of these four criteria. But I would suggest that (2), the anomaly's lack of fit with accepted theory, is the necessary element common to any real anomaly. It is a fact in search of an explanation.

This brings us to an important distinction. Coming to us from psychical research, the term **paranormal** (usually limited to psychological anomalies in psychical research) was created to designate phenomena considered natural -- not supernatural-- and which eventually should find scientific explanation but thus far have escaped such explanations (Truzzi, 1978). The term **supernormal** has been used in somewhat the same way. The clear intention of those using the term **paranormal** was to avoid using the term **supernatural**. They wished to emphasize that the phenomena was aberrant but not beyond natural science; it was simply a matter of scientific explanation catching up with such maverick facts. Unfortunately, many critics of the paranormal continue to equate anything purportedly paranormal with the supernatural. This is particularly ironic since those who truly believe in the supernatural (such as the Roman Catholic church when it speaks of miracles) have long understood that a paranormal explanation precludes a supernatural one. Following the proper use of the term **paranormal**, we can logically speak of the **parasciences** which deal with anomalies in their respective domains. Thus, we can speak not only of parapsychology but also of such fields as parasociology, paraphysics, etc. In each case we refer to the study of anomalies within some discipline for which they seem potentially relevant.

In fact, however, given a particular anomalous event, we really don't know in advance which current science will eventually develop an explanation for it. Thus, something like extra-chance correct guessing scores may now be part of parapsychology, but in the end the explanation may lie elsewhere. For example, if it turns out that such anomalies are due to some statistical malassumption that might cause us to develop some new statistical theory for the way such things are distributed in nature; the phenomenon may then better belong to parastatistics. Similarly, unidentified aerial phenomena may eventually be explained via meteorology, astronomy, psychophysiology, or some other discipline (or even by all of these in part since we may be dealing with different phenomena at different times). Thus, I would suggest the term put forward by Roger W. Westcott (1973 and 1980), **anomalistics**, to speak of

the interdisciplinary approach to anomalies of which the various parasciences are branches.

Returning now to the term **paranormal**, this term avoids our confusing true anomalies with the merely irregular or rare event that does have current scientific explanations. Such latter events are usually termed **abnormal**. Thus, a man 8 feet tall would be abnormal but not paranormal, whereas a man who could breathe water and live would be paranormal. Similarly, a virgin giving birth to a female would be abnormal (since parthenogenesis would offer an explanation for such a rarity), while a virgin giving birth to a male would be paranormal (to say the least!).

Since the key element in the definition of an anomaly is its relationship or lack of relationship to theory, this means that all anomalies are relative to some specific theory. The degree of anomalousness or extraordinariness of some event can only be specified in relation to the theory it fails to fit. The term **anomaly** specifies a relationship between an event and a theory; the event is not anomalous in and of itself. Science usually contains many theories, and they are by no means integrated or even consistent with one another. Thus, a claim like precognition may present serious problems for the physicist but few for the geologist. Not all scientists will view an event as equally extraordinary. This disunity may be functional, as when biologists ignored the physicists who said that the youthful age of the sun would make biological evolution impossible over the postulated great lengths of time. The physicists later discovered fusion and changed their estimate of the age of our sun, so in this case physics lagged behind the biologists' theories even though we usually think of physics as the more fundamental science.

I find that in the broadest sense there are three general orientations towards anomalies. For many scientists, anomalies are mainly troublesome and to be ignored or denied. They view anomalies as what Charles Fort called "damned facts." For such scientists, anomalies are at best an irritant. At worst, they view anomalies as challenges to what is fundamentally held dear and so are to be opposed, sometimes at almost any cost. The object, for such scientists, is not to explain but to **explain away** the anomaly.

The second orientation commonly found is that of the **mystery monger**, the person who relishes the fact that a phenomenon is unexplained. This, alas, is quite frequently the attitude held by many who call themselves "Fortean" (Clark, 1983). They enjoy the mystery and even gloat over the inability of scientists to account for the anomaly. They don't want to explain or explain away anomalies; they want to uncover more of them, and their goal may—in the extreme case—be anti-scientific in spirit; for they seek mysteries rather than explanations of them. In reading some Fortean, one clearly gets the impression that even an unconventional normal explanation by science would be a disappointment.

The third orientation—the one I espouse—consists of seeing anomalies not merely as challenges but as opportunities. Anomalies are actually the major source for theoretical change and conceptual progress in science. Though we should be highly cautious about accepting claims of anomalies, we should look forward to finding valid ones because it is by explaining these aberrations that we will expand our theories and create new theories. From this perspective, anomalies are important and valuable because they lead to new and greater forms of scientific explanation. Thus, anomalies should be seen as

constructive rather than destructive, as forces inspiring scientific growth and progress.

Let me turn now to some of the various dimensions or categories of anomalies.

1) The anomaly may be legitimately unexplained or paranormal rather than something at first inexplicable but actually covered by known laws, something merely abnormal (as just discussed) or a **pseudo-anomaly**.

2) The anomaly may be a scientific anomaly, something **paranormal** and ultimately explicable in terms of science, as opposed to something actually outside the natural order, something not paranormal but **supernatural** or metaphysical. The latter would include not only such things as divine miracles but also perhaps any possibly unique event in nature, a kind of cosmic hiccup or burp, something without pattern or impossible to generalize about or find lawful order in. Science is law seeking, and it is at least conceptually possible that some things may happen that are simply outside all regularities, some things I would term **preternatural**.

3) The anomaly can vary in its relationship to scientific theory. Thus, it can be **unnested**, that is, simply a stray phenomenon that seems to have no body of theory to deal with it one way or another. Or, more typically, the anomaly can be **nested**, that is, can pretty clearly be seen as something that should be covered by a certain theory even if it is not. Nested anomalies can vary greatly in the degree to which they fail to fit within the existing theoretical net. Some anomalies merely fit poorly while others seem to contradict central parts of the theory. The nested anomaly that contradicts important parts of the accepted body of theory relevant to it is the one most troublesome and most likely to be denied legitimacy/validity.

4) An anomaly may be something **spontaneous** in nature or may be something which only can be **experimentally produced**. And if it is experimentally producible, it may vary in the degree to which it is reproducible. Replication can be occasional only or vary all the way up to repeatability upon demand. In general, anomalies tend towards being spontaneous or only occasionally repeatable, and that is usually one of the reasons their very existence is typically controversial.

5) Anomalies exist to some degree in every scientific area and are usually acknowledged as such. Thus, there are **accepted anomalies** which are not controversial. These are usually viewed as minor puzzles to be solved, as not very troublesome loose ends. But most anomalies deemed interesting have a struggle to gain acceptance. Most anomalies are merely **alleged anomalies**. They remain unestablished. Some are **validated anomalies** in that most scientists might agree that they really exist; but this is unusual, and it is imperative that the anomalist recognizes that most of the time he is dealing with merely alleged anomalies.

6) An anomaly may be something that occurs rarely in nature or something that occurs frequently, even if it is a spontaneous anomaly. Frequency must be considered relative to the **temporal dimension**. Thus, something may appear to be rare but may actually have occurred on many occasions over a long period of time or, conversely, an event may occur a number of times within a short period but never happened previously or afterwards.



7) An anomaly may vary along the spatial dimension. That is, an anomaly may be widely dispersed or narrowly available.

8) An anomaly may be rarely or frequently **seen** (whether or not it occurs rarely or frequently).

9) An anomaly be rarely or frequently **reported** (whether or not it is rarely or frequently seen).

10) The anomaly may be an anomalous **thing**, or it may be an anomalous **relationship** between quite normal things. I have elsewhere termed anomalous things **crypto-events** and anomalous relationships **para-events** (Truzzi, 1977). Things, crypto-events, are relatively easily validated. You simply "bring in the body" and show it to the skeptics. But falsification of the crypto-event may be especially difficult since the thing (e.g., a Bigfoot) could have been elsewhere than where you looked. The reverse is true for the para-event. Since a relationship is something to be inferred from the data, alternative explanations are usually possible. Thus, validation for any para-event is most difficult. But we can usually agree that a certain correlation from which we would make our inference of the para-event should be obtained in a given experiment. So, if we fail to find it, that is usually accepted as a falsification of the claimed para-event. **Anomalistics**, then, consists of both **cryptosciences** (which study hidden things, e.g., as cryptozoology studies anomalous animals) and **parasciences** (which study anomalous relationships between things, e.g., as parapsychology studies anomalous psi processes), and these two branches of anomalistics have different strategic problems as they seek acceptance/legitimacy within the general scientific community.

And 11) an anomaly may appear bizarre or mundane. From a scientific standpoint, this should largely be irrelevant. But it clearly makes a difference to the way many will evaluate the degree of extraordinariness of the claim. An anomaly high on **strangeness** will have more trouble getting accepted even though it may have less trouble getting noticed in the first place.

Having considered some of the major dimensions of anomalies, let me now try to integrate some of these observations with my earlier remarks about skepticism.

### III. The Zetetic Approach to Anomalies

When speaking of the extraordinary in science, we need to consider both extraordinary events and extraordinary theories (explanations). We should first seek to explain extraordinary events in terms of ordinary theories, and only upon failing should we move on to extraordinary explanations (Truzzi, 1978a). But, alas, the anomaly literature is full of far too many attempts to explain ordinary events with extraordinary theories.

Any anomaly claim will normally consist of three elements: (1) the anomalous event itself, (2) the report or narrative about that event, and (3) the reporter or narrator of the report or narrative account (Truzzi, 1978a). Critics can attack the claim at any or all of these three points. The event can be viewed as too improbable, the narrative as too implausible, and/or the narrator as not credible. Each of these elements can be assessed indepen-

dently, and the skeptic usually begins with doubts about the claimed event itself. He then properly focuses his arguments and evidence against the narrative account. But it is common for criticism to extend to the credibility of the narrator, this sometimes taking the form of irrelevant ad hominem attacks on the narrator's motives.

Attacks on the credibility of the narrator follow a pattern. The reporter of the anomaly can be attacked through the use of a number of negative labels. He can be called a crank, a crackpot, an incompetent, or a charlatan. All these labels have been used to designate what the critic sees as a "pseudoscientist" (far too often a magic word invoked to exorcise heresy). A **crank** is simply one who tenaciously clings to a deviant or minority position. This is normally done via rational arguments and evidence. We have many cranks in ordinary as well as extraordinary science. Ron Westrum has pointed out that there are both **reactionary cranks** (those who cling to old, discarded ideas) and **radical cranks** (who espouse wild new ideas). Cranks are "difficult" persons (cranky?) but not irrational. A **crackpot** is one who supports a wild idea (also either reactionary or radical) but does so irrationally, i.e., without proper arguments and/or evidence. He is a scientific "nut." The terms **crank** and **crackpot** represent references to the argumentative style of the proponent so labelled.

An **incompetent** is simply one who makes unintentional errors, out of inability or ignorance. Ordinary science is full of incompetence, as is any professional field. But a **charlatan** is one who makes intentional errors; he is a fraud.

When the proponent of an anomaly is an insider in science (what Isaac Asimov has termed an **endo-heretic**), critics tend to be more tolerant and usually pull punches and reduce even the crackpot or charlatan in our midst to mere crank or incompetent. But if the proponent is an outsider (an **exo-heretic**), critics tend towards exaggeration, elevating the mere crank or incompetent into the role of a crackpot or charlatan.

We might also note that critics may themselves be members of the science community (what I would term **endo-critics**) or outsiders such as philosophers, science writers, or even magicians (**exo-critics**). Exo-critics seem to be even more concerned with social control over the boundary between science and pseudoscience than endo-critics. And such exo-critics may play the role of what Ray Hyman has termed "hit men" in science, brought in to discredit deviant claims and claimants and to "protect the public." At its worst, this takes a holier-than-thou (more-scientific-than-thou?) posture, and such vigilantes can act rather like inquisitors for a Church of Scientism (see the discussion in Feyerabend, 1978: 91-96).

It has been observed that acceptance of an anomaly can come about through either (1) an adequate degree of replicability (for some critics of some claims this may mean repeatability upon demand); (2) a new acceptable theory to house the alleged maverick fact, thereby reducing or eliminating its anomalous character; or (3) some practical application of the anomaly (a purely pragmatic and atheoretic approach). (3) is actually a version of (1) since usefulness implies repeatability, but the level of adequacy in replication would be lower for (1) since the atheoretical argument side-steps contradiction of existing theories.

If extraordinary claims require commensurate "extraordinary" proof, the requirements (in terms of quality or quantity, not in terms of the standards) diminish with a reduction in the extraordinariness of the claimed anomaly. And if we are to judge how anomalous a claim is, we first need to explicate clearly what impact the existence of the anomaly would have on our theories. An unnested anomaly should require less evidence than a nested anomaly. If we look closely, we frequently find that a common view of how anomalous something is may be confused with how "strange" or "bizarre" the claim may seem. Possibility of the event is confused with plausibility of the narrative (Truzzi, 1978a). Almost no one believes in unicorns today, but finding living unicorns would have very little impact on current zoology. Telepathy would be far less revolutionary for psychological theory than would be precognition, so telepathy should require less proof. And a living plesiosaur in Loch Ness would be far less extraordinary a claim than would be a living mermaid or a centaur. We need to far more carefully explicate how scientifically extraordinary a claim really is and then weigh the evidence in light of that assessment.

It should also follow that claimants can reduce need for evidence by minimizing the revolutionary character of their claims. This can usually be done by taking a more atheoretic approach when presenting evidence. Like Sherlock Holmes, we should eliminate the unnecessary elements and consider the bare bones first. We especially should try to dissociate our anomaly from any occult or metaphysical frameworks (even if these inspired our investigation). Parapsychologists keep reminding us that they are not occultists and are not part of Spiritualism. Cosmobiologists remind us that they are not arguing for traditional astrology. Some ufologists remind us that they are investigating unidentified aerial phenomena and are not proponents of identifying UFOs as extraterrestrial craft. In general, this means any anomaly should first be presented as a question rather than as part of some extraordinary answer. Exotic correlations, for example as in Michel Gauquelin's "Mars Effect" or in parapsychology's extra-chance guessing scores, should first be presented as mere correlations before leaping to conclusions about causal relations (Truzzi, 1982). Too often what might have been accepted as a legitimate puzzle is repudiated because the claimant insists he has found a fulcrum to place his lever for scientific revolution and paradigm shift.

But the claimant finds himself in a paradoxical position. By thus reducing his anomaly claim, he may trivialize its importance, and that results in its being assigned a lower priority for scientific investigation. So, the claimant is usually forced to seek attention and resources by stressing the revolutionary character of his claim. But the more important the anomaly is in this way, the greater is the degree of proof demanded; and raising the threshold for adequate evidence makes criticism easier. This often forces the proponent of the anomaly to seek support outside of science, either from the public or —worse— from occult or metaphysical supporters; this then further mobilizes antagonism among scientists.

But if the proponent is trapped in this spiral, surely the critic is not. The goal of the scientist is to explain whatever anomalies truly exist. So the critic —independent of the proponent's posture— has an obligation to examine the strongest evidence for the least theoretic version of any anomaly claim.

Finally, we need to recognize that the importance of an anomaly may be primarily extra-scientific (Truzzi, 1981). Psychologists may properly assign low priority to parapsychological research given the level of proof so far

offered. But the military and security-concerned branches of government may recognize that even such long shots would have enormous impact if even only partially valid. So, scientists may rationally assign low or high priorities based on their specific reasons for wishing to avoid a Type I or a Type II error, and opposite assessments of priority can rationally emerge from different orientations and stemming from nonscientific factors. Critics of government research into anomalies sometimes neglect to consider such factors. But so, alas, do some proponents. Thus, covert government interest in UFOs does not necessarily mean greater covert belief in the reality of UFOs. It may merely reflect government's different criteria for the importance of avoiding a Type II error. It may even be the case that such misunderstanding is partly the reason that government agencies feel obligated to keep their work covert. A similar argument applies to covert government work in parapsychology; such work does not necessarily indicate covert government belief in the reality of psi.

I have tried to consider some of the interrelationships that emerge from applying a zetetic approach to anomalies research. As always with proper zeteticism, I leave you with more questions than answers about these matters. But I think these are some of the right questions that need to be asked if we are to make progress in dealing with scientific anomalies.

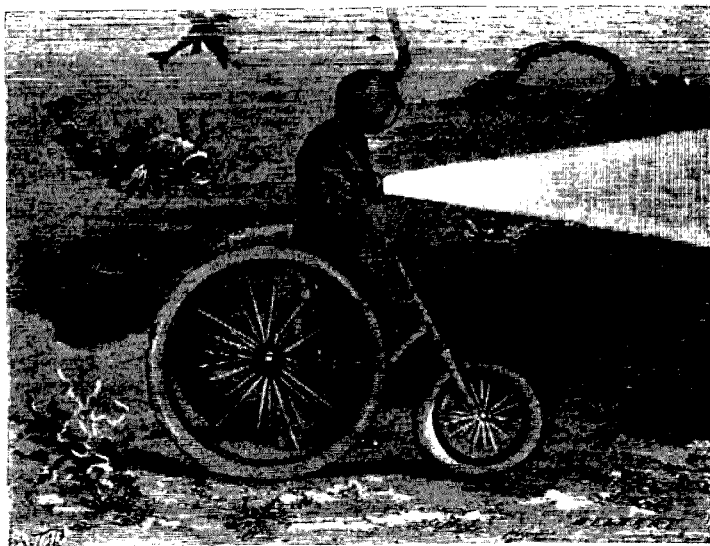
#### Note

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## A REMOTE VIEWING EXPERIMENT CONDUCTED BY A SKEPTIC AND A BELIEVER

James McClenon & Ray Hyman

"Remote Viewing" is a term used to describe the alleged ability of one individual to "view" (mentally) the images perceived by a second individual who is at a separate (and remote) location. This ability may be considered a form of extra-sensory perception since it hypothesizes the experience of a target location without sensory contact. Much controversy surrounds the ontological status of this alleged ability.

Various remote viewing experiments conducted by individuals who believe in the reality of psi (for example: Targ and Puthoff, 1977; Duanne and Bisaha, 1979; Schlitz and Gruber, 1980) seem to indicate that this phenomenon exists and requires further exploration. Other experiments conducted by skeptics (for example: Karnes et al., 1980, Marks and Kamman, 1980) show no evidence for the remote viewing hypothesis and hint that psychological factors can explain, in part, the misguided belief in psi.

It would seem that the conduct of a jointly supervised, believer-skeptic experiment might shed light on this issue. One researcher (R.H.) is a well known skeptic regarding claims of the paranormal. The other researcher (J.M.) instigated the experiment in order to play the role of a "believer" as an aspect of a sociological participant observation study of the field of parapsychology. This experiment was designed not merely to test the hypothesis concerning the ontological status of remote viewing, but to uncover the various social-psychological factors that might be associated with its experience. We hoped to gain information about this aspect from the subjective and informal reports of the experimenters and also the subjects. The experiment, as set up, can provide only a formal test of the psi hypothesis.

The experiment described in this paper involves eight remote viewing trials using eight different subjects and was conducted during the spring of 1980 in Eugene, Oregon. The experiment was conducted as a replication of the successful remote viewing experiments of Puthoff and Targ (1976) and Bisaha and Dunne (1979). In addition to using independent judges, we also had subjects judge their own protocols as is done in Ganzfeld research (Honorton, 1977). A distinct target pool was developed for each subject in order to avoid methodological problems encountered by previous researchers during the judging process.

### Method of obtaining subjects

Five "inexperienced" volunteers, i.e., ones having no previous experience regarding remote viewing experiments, were obtained through the placement of a classified ad in the University of Oregon newspaper. Three other "inexperienced" individuals volunteered during informal interaction with one of the researchers (J.M.). Although the subjects were "inexperienced" in remote viewing, only one was naive or inexperienced in occult matters. Parapsychologists might label the other seven as "sheep."

All volunteers were asked if they knew anyone with whom they wished to work in this remote viewing experiment. The experiment requires an "agent" who attempts to mentally "send" an image to the "receiver." All the volunteer "receivers" decided to use the available researcher (J.M.) as the "agent."

#### Apparatus

One researcher (J.M.) took photographs of 64 locations in the Eugene area. The number of photographs taken of each site was based on the researcher's judgment of the complexity and size of the possible target. This was the same photography procedure used by Dunne and Bisaha (1979). Notes were taken on 3x5 cards describing the observer's exact location, the visual direction to be faced by the observer, and the objects which were to be observed. The 64 site photographs were arranged into 8 sets of 8 sites. An attempt was made to create 8 pictorially distinct possible targets within each set with regard to architecture, objects within the photographs, general forms, colors, etc. This allowed each set of 8 sites to be assigned to each of the 8 trials. A xerox copy was made of the photographs of all the potential target sites.

The potential target sets were numbered from 1 to 8 and each site within each set was numbered from 1 to 8. The entire package was given to an individual (O.B.) who was totally unconnected with the rest of the experiment. O.B. assigned a second individual to arrange the target sets randomly and to select a target site within each set in a random manner. The PDP-15 computer within the cognitive laboratory at the University of Oregon was used for this random selection process. O.B. had the photographic material arranged in such a manner that he would remain unaware of the target number until the beginning of the trial run. He was always unaware of the target site.

#### Procedure

Each of the 8 subjects were tested on different days. The subjects remained with the observer while the remote viewing agent (J.M.) went downstairs to the computer area. Here the agent received the target materials from O.B. These consisted of a 3x5 card describing the target site and a xerox of the target photographs. The selected set of 8 sites was then brought up by O.B. to the observer and subject after the agent had left for the target site. Only the agent, who was on his way to a specific site, knew which site was the target. R.H. was the observer in all the trials.

The subject was allowed a 20 minute relaxation period during which the agent traveled to the target site by bicycle. The subjects were allowed to prepare themselves in whatever manner they wished. Most employed forms of meditative procedure while a few spent the time talking with R.H. After this time period, the subject described aloud into a tape recorder the images that came into his or her mind during the next 15 minutes. Paper and pen were available to make sketches if desired. At the end of the 15 minute time period, the subject was shown the photograph of the 8 sites and requested to rank order them as to their similarity to his or her previously described (and tape recorded) description. The subject remained closeted with the observer during this time period. Following the termination of his 15 minute stay at the target site, the agent returned to the closeted subject and observer. He then notified the subject and observer of the correct site. The subject was then debriefed. The researchers were

especially interested in changes that might occur in the subject's belief system regarding psi.

#### Judging

Two forms of judging were planned. The first series involved having each subject rank order his or her 8 photographs regarding their similarity to his or her 15 minute verbalization. The score achieved by each subject was the rank that the individual assigned to the photograph that was later found to be the target site. These scores are listed in Table 1.

A second series of judging involved other individuals, who were geographically distant from Eugene, Oregon. The transcripts were typed by an individual who was unaware of the target site for each trial (this would prevent bias from intruding into the typist's interpretation of words which might be less than completely clear). Dr. Stanley Drippner, at the Humanistic Psychology Institute (presently the Saybrook Institute) agreed to instigate the "remote judging for this experiment. Eight envelopes, each containing the 8 site photographs and each containing a typed transcript were mailed to Krippner's assistant, John Geyl. Geyl located five individuals who were asked to rank order the photographic material for each trial. They used the typed transcript of the subjects' descriptions recorded during the designated 15 minute intervals. The judges' scoring on the target site for each trial was averaged and is listed in Table 1.

Table 1  
Remote Viewing Experiment Tests of Significance

Trial Number	Subject's Score	Judge's Score (Average)
1	3	4.0
2	6	6.4
3	2	2.6
4	7	4.2
5	8	5.2
6	6	7.2
7	7	6.0
8	1	2.6
Mean	5.0	4.8
	Not Significant at .05 level	Not Significant at .05 level

## Results

Table 1 presents the subjects and average judges' score for each of the 8 trials (derived from the ranking assigned to the target site). A statistical evaluation of the subjects' total score can be conducted using Table A-1 of Solfvin, Kelly, and Burdick's (1978) article regarding the methods of analysis of preferential-ranking data. This method of analysis, the ordinal weighting scheme, reveals the improbability of achieving any sum of ranks. The smaller the sum of ranks, the more improbable that this score could have occurred by chance.

This method was applied to the average rank for the subjects and each of the judges separately. A regular T-test was applied to the averaged judges' rankings, as well as the combined scores of judges and subjects. None of these tests achieved statistical significance.

Table 1 reveals that the subject's total score does not demonstrate evidence for the existence of a remote viewing facility. Each of the five judges, taken singly, fails to come up with a significant sum of ranks. The subjects' rankings correlate with the average rankings of the judges approximately 0.74, which is significant at .05 level.

The combined scores of judges and subjects yielded a mean ranking of 4.81 with 95% confidence limits of 3.65 and 5.97. Because the average ranking expected by chance is 4.5 our results are fully consistent with the complete absence of psi in the subjects' target descriptions. (The error term used to construct the confidence units was derived from the repeated-measures analysis of variance on the judges' and subjects' rankings).

## Discussion by J.M.

In order to properly play the role of "believer," I decided to conduct various investigations outside of the domain of the remote viewing aspects of this experiment. The hope was that these investigations would shed light on the nature of the psi phenomenon. Believers (among the parapsychological community) are generally more interested in experiments that attempt to be "process" oriented rather than those that are oriented only toward "proving" psi. These additional investigations consisted of (1) attempting to evaluate my personal clairvoyant ability on the day of each trial in order to determine the possibility of a relationship between agent clairvoyance and subject RV ability and (2) interviewing each subject before and after the subject's trial in order to determine the effect that participation in the experiment might have on each subject.

Previous to taking part as the agent during each trial (and to having any contact with O.B., the randomizer), I attempted to predict the target site number. Each day, while in a meditative state (induced through a method of progressive relaxation), I rank ordered the numbers between 1 and 8 as to my feeling of the probability of each being the target site number. In this manner a prediction series was generated in the exact same format as that which would be later generated by the subject. The possibility of sensory cuing could not enter into this aspect of the experiment since O.B. had his target information arranged in such a manner that he remained ignorant of future target site numbers on each trial day. These predictions were written each day on a 3x5 card which remained in my possession. O.B. was

aware of this aspect of the research before the experiment began and R.H. was informed after the completion of trial 2. This separate clairvoyant test has no bearing on the formal test of the remote viewing hypothesis.

Table 2

<u>Trial Number</u>	<u>J.M.'s Score</u>
1	6
2	1
3	4
4	1
5	2
6	2
7	2
8	3
Mean	2.6
p=0.012 (one tailed)	

Table 2 presents the results of my clairvoyance experiment. My total score reveals a level of significance of 0.012 (using the same Solfvin, Kelly, Burdick (1978) table as was used in the RV aspect of the experiment). This one tailed value was selected in order to maintain equivalency with the remote viewing analysis. Some parapsychologists (and also critics) might argue that, due to the nature of psi, two tailed tests are always required. In the case of my clairvoyance experiment, this would yield a p value of 0.024.

There appears to be no relationship between my score each day and the subject's score. The correlation between my scores and the scores of the subject is -.68. My scores correlate with the average of the judge's scores -.53. Neither correlation is significant at the .05 level (two-tailed). Since this is an "informal" aspect of the RV experiment, I will include my discussion of these results within the discussion of changes in belief among the RV experiment participants.



Interviews with the subjects before and after each trial revealed that some individuals changed their attitude toward psi as a result of participating in the experiment:

#### Trial 1

S-1 was a former student of R.H. and maintained an open-minded skepticism concerning psi. S-1 seemed to increase his belief in the possibility of the existence of psi after finding that he had ranked the target site in the third position. The subject's description of numerous facets of the target which were either not prominent in its photograph or outside of the borders of the picture seemed more than coincidental. S-1 claimed a continued open-minded skepticism but "an increased interest in future experiments."

#### Trial 2

S-2 experienced a situation which might be expected to decrease belief; he reported, "It hasn't altered my belief. I still believe pretty much." S-2 rationalized his apparent failure while talking to J.M. "I was thinking about where you would be instead of letting things just pop in my head."

#### Trial 3

S-3 experienced a situation (achieving a score of "2") in which her belief might be increased. She reported, "I've always thought this kind of thing could work. I generally believe that this can happen but I'm not totally confident in it. My belief stays about the same."

#### Trial 4

Although S-4's performance on this test might be expected to reduce her belief in psi (she ranked the target site in the seventh position), her numerous anecdotal stories from personal experience supported her continuing, undiminished belief in psi.

#### Trial 5

S-5 stated previous to the experiment that he felt that the demonstration of psi was very possible. After his session, he stated that he felt he had had great difficulty in clearing his mind of his normal residue of personal thoughts. Following his unsuccessful trial (he achieved the worst possible score, and 8) he stated, "I was a believer before and I still am. Some people are more receptive than others. Just because I didn't get results doesn't change anything. If I see your ad in the newspaper again, I'll help you out by not replying." (S-5 laughs after stating this). "This one trial doesn't prove anything."

#### Trial 6

S-6 related various anecdotal evidence of psi in her personal life. Following her unsuccessful attempt to demonstrate psi under experimental conditions (she achieved a score of 6), she reported: "It's depressing. I still know it works; I'm just not that good at it." She then went on to describe several more personal psi experiences which had occurred previously.

#### Trial 7

S-7 believed in the efficacy of astrology and was a believer in psi. Her protocol consisted more of a personality reading of the agent (J.M.) than an attempt to describe his location. Her inability to accurately choose the target photograph disturbed her only slightly (she placed the target site in the seventh position). She was convinced of the accuracy of the personality reading. Personal friends of J.M. also believe that she gave an accurate reading.

#### Trial 8

S-8, a believer, was very extroverted and outgoing. Although she seemed to be a perfect psi subject, both experimenters were aware that it would be impossible at this stage for the remote viewing aspect of the series to demonstrate statistical significance overall. S-8 achieved a direct hit on the target photograph and described numerous similarities between her protocol and the target picture after being informed of her accuracy. R.H.'s notes of the experimental protocol reveal that almost immediately after the tape recording of the session ended, she remarked that she "kept thinking he was at the downtown mall." When one of the target photographs was indeed of the downtown mall, she felt certain that this was the target and she was correct. S-8 stated that her belief in psi increased due to her experience. Her success was an exhilarating experience for her.

R.H. and J.M. remained interested but both recognized that future judges would find it difficult to rank her transcript. Many of her remarks concerned activities and emotions of the agent (that were later verified) which were of no value to the future judges. Although S-8's comment concerning the Eugene mall appeared in R.H.'s notes, it was not entered into the typed transcript since it did not occur during the 15 minute time period. Later, the judges in San Francisco were apparently unable to note an extreme similarity between the typed transcript and the target site. Their average ranking of this trial was 2.8.

It would seem from these observations that an individual's evaluation of psi performance is dependent on the past experience of the individual and on the person's "frame of reference." S-8 should not be expected to dismiss her own successful experience because others had failed to obtain remote viewing results. Rather, it would be more logical for her (from her personal vantage point) to increase her belief in the methodology that she utilized (belief in God, goodness, etc.). The fact that she had chosen the correct site from a possible set of only eight locations did not diminish her achievement in her mind. She reasoned (probably correctly) that the experimenters could have supplied her with a hundred possible sites. She still would have chosen the correct one. My belief regarding my personal clairvoyance "success" follows a similar pattern. Although the RV results indicate that my belief in the RV methodology should be reduced, my clairvoyance results indicate that my belief in deep relaxation and meditation as a means for eliciting clairvoyance should be increased (at least to a slight degree). These changes in belief might be logical from the vantage point of a "believer" yet less logical for a "skeptic."

This observation regarding belief in psi seems to vaguely support some of the observations noted by Marks and Kamman (1980) regarding the psychology of the psychic. There does seem to be a tendency for an individual to pick out aspects of what he or she observes and to use these aspects to reinforce a system of belief which has been developed in the past. At the same time, my observation of the subjects during this RV experiment revealed little that seemed irrational within their comments. Indeed, they manifested few of the irrational aspects that Marks and Kamman (1980) observed, but appeared to be typical college students who happened to believe in psi because of their past experiences.

Rather than argue about the ontological status of psi, it might be more fruitful to consider the means by which an increasing percentage of the population has come to believe in it. Social phenomena, such as belief in psi, can be accepted by both believer and skeptic.

At the heart of the psychological aspect of this issue is the question regarding the nature of experiences that induce belief in psi. Some skeptics have presented simplistic analyses that reflect their belief that psi does not occur. For example, Singer and Benassi (1981) seem to attribute belief in psi to "deficiencies of human reasoning" and to "faulty cognitive apparatus." Since personal psi occurred for me under the conditions that I devised, I feel that I might shed light on this issue. While meditating, I evaluated my feelings with regard to the possible target numbers. Sometimes I felt numbers "popping into my head" and found that these numbers frequently were correlated with the actual target numbers. It does not seem to me that either my clairvoyant success or S-8's RV success can be attributed to "faulty cognitive apparatus." The "deficiencies of human reasoning" may be a more valid theory for explaining belief. The tendency to focus attention on memories that coincidentally coincide with "target" objects, states, or events could create the illusion for the experiencing individual that psi has occurred. Consequently, I cannot claim that psi was "proved" by my experience, but I do claim that my belief in psi was increased by it. Various factors explain this phenomenon:

1. The small number of rests of significance (preset as one-tailed tests) that were planned and the high level of significance obtained on the clairvoyance test.
2. The great degree of care which was exercised by the randomizer, O.B. (who was a skeptic) in order to preclude the possibility of sensory cuing.
3. The emotional experience of feeling the numbers "popping into my head" and the finding that this "popping" process was a successful means of gaining information. This experience was compounded by the fact that during my mediation for trials 6 and 7, I felt a high degree of certainty that one number would be correct but later in the day decided that it would be "better" to choose another number. In both of these cases, the first choice proved to be correct. At the end of the series, I felt a degree of relief that I had not obtained an extremely high level of statistical significance. Such an event would force many skeptics to accuse me of fraud. With the present level they will probably dismiss this result as a case of selective data analysis (for example, see the discussion of this aspect of the experiment by R.H.)

I would suggest that my personal experience, that of S-8, and, to a lesser degree, S-2, is similar to the spontaneous experiences that seem to be a major source of the high level of belief in psi that exists within the general population (McCready and Greeley, 1976). Some of these experiences are quite powerful. An individual who sees an apparition of a relative and later finds that this relative died at the very moment should not be accused of "deficiencies of human reasoning" or "faulty cognitive apparatus" if he or she increases belief in psi as a result of the experience. R.H. and J.M. may explain to S-8 that her experimental result does not "prove psi is real," since the other subjects were less successful. R.H. may present an analysis to J.M. (using aspects of the data that fail to support the psi hypothesis) showing that statistical significance would not be demonstrated even if his clairvoyance test had been built into the formal experiment. A skeptic might explain to an individual that apparitions are results of "faulty cognitive apparatus" and that to believe that a relationship between such an apparition and the death of a relative is more than coincidental demonstrates "deficiencies of human reasoning." These arguments seem rational to the skeptic, yet from the vantage point of the experimenter, the impact of the experience negates these explanations. Such experiences, although they can not be granted the evidential quality of results derived from formal tests, still generate belief in the existence of psi. My argument is that such modification of belief should not always be deemed irrational, especially when viewed from the framework of the "believer."

The problem lies in the different assumptions inherent within the believer and skeptical positions. It makes these orientations incommensurate. Believers have difficulty explaining the inability to demonstrate the existence of psi in a consistent manner. They must resort to explanations that involve expectancy and researcher effects (White, 1977). They find it easy to explain why people have spontaneous experiences which lead to belief in psi since they feel psi produces these experiences. Skeptics, on the other hand, have no difficulty explaining the failures within parapsychological experiments. They believe that psi probably does not occur. Their problem lies in devising an explanation as to why so many people have had experiences that lead to belief in psi's existence. Their present theories do not stand up under empirical testing.

In considering the investigation of controversial claims, both skeptics and believers engage in a process of selective observation and interjection. Will our claim of failing to support the RV hypothesis reduce belief in psi among believers? Probably not. Their past experiences have led them to believe that we did not induce the "proper conditions" for psi to occur. In controversial areas, observers tend to ignore or re-evaluate results that do not coincide with their previously formed opinions.

When experiences relevant to an inquiry occur outside of the formal experimental format, is it rational to grant them a degree of validity? Of course it is! Yet because of the conditions under which the experience occurred, much of the rhetorical power to persuade others concerning the authenticity of the experience is lost. It is rational to believe one's own experiences, but less rational to grant equal weight to the description of other's experiences. Scientists use their own experiences, which occur both inside of the laboratory and out, to evaluate reports of other's experiences, which occur inside of the laboratory and out.

A major problem is the question of "rationality" and the extent to which a universal rationality can be assumed. Both believers and skeptics present "rational" arguments, but this "rationality" is derived from the speaker's initial assumptions. Theoretical orientations devised within the "relativistic" sociology of science explain this dilemma:

It would seem that evidence is so bound up with the sociology or social group which gives rise to it that theories held by members of radically different scientifico-social groups cannot be adequately tested against each other by experiment. It matters not whether the evidence is intended to corroborate, "prove" or refute the theories in question. Similarly, these differences cannot be settled by logical argument (Collins and Pinch, 1982:184).

This does not mean that science cannot resolve issues such as the question regarding psi, but that such resolution will not be brought about by any single series of experiments. Such issues are eventually resolved through the rhetorical and political processes or argumentation that constitute science. If this issue is to be resolved, the question must be reformulated in such a manner that arguments will convince major elements of the scientific audience regarding the validity of the new claim. Parapsychologists might present their "expectancy" or "experimenter effect" theories in such a manner that they can be empirically verified by skeptics. When this is done, such "failures" to replicate the psi hypothesis as occurred in our experiment may be deemed as "successes." Skeptics might devise hypotheses, that can be empirically verified by believers, that explain why such a high percentage of the general population reports experiences that lead to belief in psi. McCready and Greeley (1976) found that 58% of the American public claim to have had an ESP experience, for example. Observations within this present experiment hint that incorrect reasoning cannot account for all experiences that lead to increased belief in the psi hypothesis.

The content of some paranormal experiences have universal aspects that transcend their cultural interpretation (Hufford, 1982). This hints that some portion of the folklore regarding psi (which has been developed both through scientific experimentation and popular experience) is more than likely associated with accurate observations that have been interpreted in a logical manner. It is hoped that the scientific process of argumentation will eventually uncover which aspects of this folklore are valid. This will require individual researchers to transcend the "culture of disbelief" that seems inherent within modern science. Researchers cannot assume that some observations are invalid merely through *a priori* reasoning. Investigators must seek a better knowledge of the experiences lying behind belief in the paranormal and consider the role of these experiences in causing that belief.

#### Discussion by R.H.

As indicated, in J.M.'s discussion, it could be argued that because we employed only 8 trials, our experiment lacked sufficient power to demonstrate psi even if it were operative. However, it is unlikely that psi was operative, but at a low level, in our experiment. If pure chance were operating, the expected average rank would be 4.5. Our eight subjects, in judging their target sites, averaged 5.0, which is just barely below expectation. Furthermore, there was only one direct hit, which is also right at chance level. The five judges, who supplied complete data, produced an overall average of 4.8

which is reasonably close to the expected 4.5. Furthermore, among the 40 separate scores for the 5 judges, there were five direct hits, which is exactly the number expected by chance.

The only hint that anything like psi was connected with our experiment is the results of J.M.'s separate predictions of target sites. Taken by themselves, they yield a probability of 0.024 of having this degree of departure from the expected value on the assumption of chance. However, they cannot be taken by themselves. In the first place, as J.M. correctly acknowledges, this aspect of the experiment was not part of the original design. As such the probability figure for it is meaningless. What if, after the fact, we found that O.B. as well as each of the subjects also had kept their own secret lists of predictions? Would we also test each of these unplanned sets of scores separately as if each were the hypothesized dependent variable? Obviously, only the tests planned and taken into account in the design of the experiment can have meaning. Even so, we were somewhat careless in specifying in advance which patterns of results would have led us to conclude in favor of psi. We tested the average rankings of the judges and the separate rankings of the subjects separately at the 0.05 level criterion. But what if one of these had come out "significant" and the other had not? What would we be permitted to conclude? And what about the fact that five separate judges produced complete data? Should we test each judge separately or treat them as a composite? We have a number of options, each of which could be convincingly justified after the fact. However, as the options increase, the chances of obtaining a "significant" result according to currently employed criteria goes up greatly.

We are fortunate, in this case, that all 5 judges and the subjects were consistent in providing us data that were non-significant no matter which of a number of data analyses we might have tried. I say "fortunate" because if the results had turned out inconsistent in the sense that some judges had provided highly significant scores, or that the subjects had differed from the judges significantly, the two authors might have found themselves sharply divided on what the results indicated about psi.

We see that even with a simple experiment such as ours, the number of options for testing psi are many, and this makes it essential that the exact outcomes which will be taken as evidence for psi be carefully specified in advance of each experiment. My recent readings in the parapsychological literature indicate that this standard is consistently violated. Now, what should we have done if, say, we decided to build J.M.'s target guesses into the original experimental design? One reasonable option would have been to specify that we would restrict ourselves, say, to just three tests of significance: (1) a test on the sum of ranks for the subjects' scores; (2) a test on the sum of ranks (averaged) on the judges' scores; and (3) a test on the sum of ranks for J.M.'s scores. We would also plan to make these tests as two-tailed because it is common practice in the psi literature to test for "psi-missing" as well as "psi-hitting." And, wishing to keep the overall error rate for the experiment no higher than 0.05, we would make each of our 3 tests at the 0.05/3 or 0.0167 level of significance. Having done this, if we obtained the results we actually did, then none of our three tests, including J.M.'s results, would have been judged significant.

An important innovation, which I credit to J.M., is the use of a separate target pool for each subject. As he points out, this avoids many serious problems of independence among trials in remote viewing experiments. This is not the place to discuss the many ways such non-independence in remote viewing experiments cast serious doubts on their statistical findings. I strongly recommend to future researchers in this area that they make every effort to employ this innovation.

I agree with J.M. that the important contribution of our collaboration lies in the informal aspects that deal with the social-psychological consequences of participation in such an experiment both by the experimenters and the subjects. J.M. makes it clear how his own orientation has enabled him to seize upon various outcomes and personal experiences which reinforce his previous tendencies to believe in psi. The interviews with our subjects further illustrate the persistence of prior beliefs regardless of the outcomes of their trials. And, of course, I, the skeptic, found everything in our joint venture consistent with an outcome consistent with chance combined with strong psychological tendencies to discover meaning in every post-hoc departure from a chance pattern.

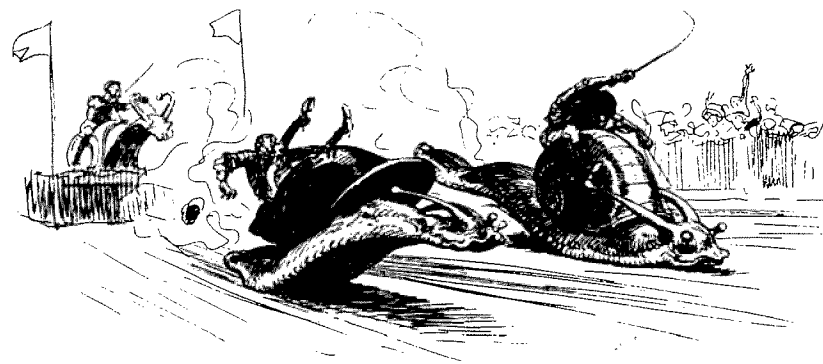
#### Conclusions

A remote viewing experiment was conducted in which subjects judged their own protocols by rank ordering individual target pools. These same protocols were later rejudged by individuals who were geographically distant from the experimental site. In neither case did statistical analysis of the results reveal evidence for the existence of a remote viewing ability. This would indicate that, within the context of this experiment, either the remote viewing ability was not present or that, given only eight trials, it was occurring in such a slight or sporadic manner as to be undetectable.

On the other hand, various individuals increased their belief in psi as a result of this experiment. This indicates the capacity of experimental procedures associated with remote viewing to induce experiences which increase belief in the existence of psi. In that psi is a phenomenon that is accepted by a large percentage of the general population, it is a legitimate scientific endeavor to attempt to gain a greater understanding of the means by which such beliefs come about. Individuals who believe in psi and those who remain skeptical of the reality of this phenomenon can both participate in attempting to uncover the factors that surround these experiences both in and out of the experimental situation.

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# NEW ZEALAND PROPHECIES EXPOSED AS A HOAX

Richard Kamman

In the previous Zetetic Scholar paper (Kamman, 1983) I reported on the case of a New Zealand psychic who on March 10, 1982, correctly forecast four major events:

- Sinking of two warships in the Falkland Islands.
- Abrupt cancellation of a major factory planned for Dunedin.
- A mid-year scandal involving the Prime Minister of New Zealand.
- The death of Soviet leader Mr. Brezhnev, predicted to within ten days of the event in a registered letter.

I can now report that, as a scientific anomaly or paranormal happening, the whole case is a hoax. I am in a good position to know because I am "Emory Royce," the alleged prophet in the report.

Before I explain how the hoax was done, I have to explain this important point. My previous story was entirely fair to readers in the sense that everything I quoted from the radio and TV interviews, and from the newspapers, was accurate. The prophecies were made as recorded, and were subsequently "confirmed" as recorded in the radio and TV transcripts and newspapers headlines. The only material that I deleted was my own name and the fact that the original episode was not a hoax in New Zealand because the radio and TV commentators explained at all stages that I am a skeptic towards the paranormal and my prophecies were offered only to debunk astrologers and psychic fortune tellers.

In writing up this episode for readers outside of New Zealand, I chose initially to hide the skeptical origin of these bogus prophecies to allow readers to experience them as they might be presented by paranormal advocates. Although the predictions drew widespread attention in New Zealand media, their impact was undoubtedly dampened by their honest portrayal as an anti-astrology lesson by a skeptical psychologist. For a proper evaluation, it was therefore necessary to hide their true origin for at least one public presentation. Even there (in ZS #11) I constrained myself to scientifically accurate reporting and quoted my own original wordings of the prophecies and their fulfillments from the tapes of the radio programs. For the miracle mongers of the paranormal press, such loyalty to the facts would be considered a bad precedent.

Zetetic Scholar, #12/13 (1987)

And now for the explanation. The methods used fall into two categories, major and minor.

## MAJOR METHOD

pure chance (includes ambiguity)

## MINOR METHODS

inside information  
trickery  
data selection

So central is the role of chance or coincidence here that removal of the other three methods might have made little overall difference to the final effect.

Here is how it happened. I was having a battle in the newspapers with a local astrologer who I believe had pulled off a fraudulent stunt on a TV program in Dunedin. The day after this astrologer gave a rather dull radio interview, I was invited to the same studio to comment on his performance. The night before, I thought it might be fun to offer some generalized predictions in the style of Jeanne Dixon and such folk, and I cooked up a few prophecies off the top of my head. It was my hope that if even one of these prophecies found a matching world event, I could later claim a success. I was also confident that if nothing happened, the radio folks would forget all about it, so there was no threat of embarrassment.

## The Prophecies Reviewed

The idea that I predicted a "naval disaster" and the sinking of two ships is nonsense. I was mainly thinking of the Three Mile Island nuclear accident when I free associated, "man-made disaster...structure bathed in or surrounded by water...poisonous gases spreading some distance and seriously affecting life... possibly nuclear...war scare." This I felt could also cover major oil spills, collisions at sea, and other situations only vaguely visualized. The "war scare" covered all military variations but could easily be dropped out.

The "sort of scandal in the government" was equally a shotgun blast at the wall, and as with certain other prophecies contained a bit of wishful thinking since I do not much admire the current Prime Minister. Ironically, I meant to design a number code that, whatever else it covered, would decode as "Prime Minister, Robert Muldoon" but I made a careless mistake and produced the code 14.13 when I should have said 13.13. Consequently, when the Quigley affair erupted, I had to wrack my brains to get anything to fit the number 14, and it remained an awkward point as John Jones noted.

The demise of the Dunedin aluminium smelter was also a case of wishful thinking, since I have opposed it all along on both ecological and economic grounds. Here is where the bit of "inside information" came into play, as my close contacts with my ecologist friends confirmed for me just then that the



smelter was, as one economist had forewarned, economically untenable. Luckily, the newspapers kept reporting the official propaganda from the investing companies and the Government, so the headlines looked very positive for a smelter in that period. Informed opinion was to the contrary.

The final prophecy, the death of a senior world leader coded 10.10 was yet another broad brush stroke--some senior world leader or other was bound to "come to an end" (not necessarily die) in 1982, and given my to's and fro's about the "eighth month" I was covered from August to October, not counting "near misses" (as the Brezhnev case was, occurring in November). Although it appeared in June that John Jones committed me to "October," just think how easy it would have been to go back to the original prediction and emphasize the eighth month for August.

I admit that Brezhnev was actually my first expectation in this case because of his age (75) and recurring reports of serious health problems. The end of his life was so likely to occur soon that I had to dramatize the prediction in his case with a registered letter. Obviously if some other senior world leader came to an end in my time zone, I could conveniently forget the letter or, if reminded, could bemoan the fact that the cleaning lady had thrown it out.

The number code "ten dot ten" could also be forgotten but the flexibility of numerological thinking would give me many chances to make a match with any famous name that should come up. After all, a lot of names might add up to twenty letters given a choice between formal names and nicknames, and using a title or not. Not accidentally did I check to make sure that another very senior world leader would permit a match: add up the numbers for letters in RONALD and again in REAGAN--surely it is "no coincidence" that both sums (64, 46) contain the same pair of digits (!) and both add up to ten, hence, ten dot ten. Violas.

In my article I stated that the name "Brezhnev" was actually inside the registered letter, and that is true. I could honestly state that no magician's trick was used to insert the name at a later stage. Basically the registered letter was a "long shot" that happened to hit the mark, but there was another little bit of trickery that increased my chances by one notch. It is also true that the registered letter would have identified the name Ronald Reagan just as clearly as it named Brezhnev, but these were the only two names covered by this registered letter effect. (The method is not given here.)

So there you have it, four predictions loosely worded and capitalizing on commonsense probabilities, molding themselves quite easily to one news event or another, and each destined to find a "best match"

But I have omitted to discuss the fifth prophecy which I blandly dismissed in my report saying it had no time boundary and so was not a usable test case. Such poppycock--it was covered in the same overall time boundary as all the others, that is, some time in 1982. It was a complete miss, partly because it was a poor choice reflecting my lack of experience with this game. In any case, the media people all followed the natural but very unscientific human tendency to overlook "non-events" and never mentioned that failed prophecy when reporting my four successes. Omitting this fifth prediction is what I meant by listing "data selection" in the methods. There was also a lot of semantic retrofitting between ambiguous prophecies and ambiguous world events, and this is another kind of micro-level data selection, or even data revision, which is the very basis of the subjective validation process. Thus, we have to allow for semantic play with ambiguities when citing chance as an overall factor.

#### Final Comments

Competent students of the paranormal are well aware of the combined effect of ambiguity, coincidence, and data selection in the manufacture of alleged miracles of precognition. I was pleased when one of the TV channels in New Zealand reviewed, at the end of 1982, the predictions made a year before by Auckland astrologers and revealed that they were a complete bust--not one was even remotely confirmed. In the light of the present demonstration, such a complete failure is difficult to achieve and requires a high concentration of poor judgment.

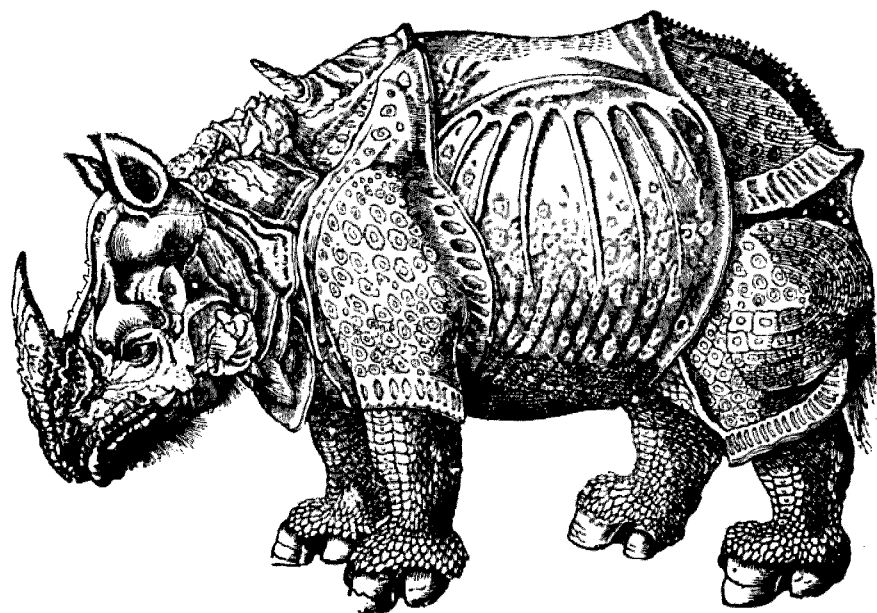
Some true believer is sure to come along now and say that I am "really a psychic but don't know it." Since there is no independent evidence whatsoever to suggest I have any psychic ability, such a claim is meaningless. It could be applied without restriction, even to weather forecasters and business economists, to claim that anybody making any prediction by any known scientific method is "really" using psychic powers. When a theory thus covers all cases and cannot be refuted by any evidence, it is empty of meaning.

Skeptics might be intellectually satisfied that alleged miracles of prophecy can be explained away in terms of subjective validation, coincidence and data selection, but it is also necessary for us to go the extra step and demonstrate that these factors are adequate to produce a few "equivalent miracles" for the world to inspect. This is a fresh application of the method of the delayed control group that David Marks and I used extensively in The Psychology of the Psychic to expose many fallacies in psychic claims. The method states that if we can go through a psychic routine using perfectly normal and scientifically known techniques to create the same pattern of results as that produced by an avowed

psychic, then we can dismiss the paranormal or supernatural claims as being totally unnecessary for that case. People are not always such bad scientists intuitively, and their requirement that we prove our non-paranormal explanations by matching the miracles of mystics and seers is a good test.

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## UFO (FLYING SAUCER) GROUPS: A LOOK AT BRITISH MEMBERSHIP

Shirley McIver

Popular interest in reports of unidentified flying objects (UFOs) or flying saucers began at the end of World War Two, and since then, social scientists<sup>1</sup> have largely been concerned to explain why people report seeing UFOs and why they believe them to be extra-terrestrial entities. For instance, using 1966 U.S. Gallup Poll data, Warren confirmed the hypothesis that those who said they had seen a UFO were status inconsistent (such as people with high education and low income) only to fail to replicate his findings in subsequent studies<sup>2</sup> and Westrum, in an analysis of the 1973 U.S. Gallup Poll data, found that those who claimed to have seen UFOs were little different from the general population.<sup>3</sup>

As far as the belief that UFOs are extra-terrestrial entities is concerned, Resta found a statistically significant relationship between the degree of externality (the extent to which a person feels powerless to control their own destiny) and strength of belief in UFOs<sup>4</sup> whilst Littig found a significant correlation between strong affiliation motivation (the desire for friends) and belief in UFOs as extra-terrestrials.<sup>5</sup> However, Fox found no support for psychological and social psychological explanations of why people believe in UFOs and argued that a more adequate explanation lay in the fact that:

"Belief in flying saucers is consistent with the United States world view and has emerged as a collective attempt to understand ambiguous and problematic stimuli."<sup>6</sup>

The remaining explanations of the phenomena have tended to be more general, considering belief in UFOs to be part of a wider expansion of interest in the occult and paranormal or as one of the many new religious movements which have emerged over the last twenty years.<sup>7</sup>

What place does organized interest in UFOs occupy in modern society? Most research on groups interested in UFOs has been concerned with those which claim to be in contact with extra-terrestrial entities. Festinger, Reicken and Schachter, Jackson, Buckner, Wallis, Balch and Taylor, and Stuppel and McNeece, all studied groups which were organized around "contactees," that is, individuals who claim to have contacted alien entities.<sup>8</sup> All of these groups were categorized as cults, although The Aetherius Society was also classed as a mystagogic congregation and later a sect by Wallis.<sup>9</sup>

This concern with contactee cults has encouraged the view that UFO groups are largely religious phenomena; indeed they are often considered to be a type of cargo cult.<sup>10</sup> Obviously this view will inform theories about the wider cultural trends as well as those about the UFO movement, yet this may be an inaccurate or incomplete picture in so far as it is

based upon the study of only one among many types of UFO groups.

For instance, Michael Schutz distinguished three major types of groups in the UFO field in the U.S.A. which he classified as "religious cults," "platform societies," and "investigations groups."<sup>11</sup> There also seem to be less well defined groups or "collectivities" as is suggested by Stupple and Dashti's study of subscribers to the Saucerian Press.<sup>12</sup> In addition, there is an organizational structure to the subject of "ufology," often mentioned in popular books on UFOs and defined by Joseph Blake as "the study of unidentified flying objects as elements in an independent theoretical-conceptual scheme."<sup>13</sup>

It might be thought that secular UFO groups are in a minority when compared with contactee cults, but this does not appear to be the case in Britain. For example, a follow-up of addresses given in a UFO directory produced twenty-nine different publications by British groups, which indicated that UFOs were important to them.<sup>14</sup> In only eight of these were UFOs regarded as connected with spiritually advanced extra-terrestrials who were transmitting messages to human beings and in these publications, the messages or "teachings," were of primary importance. In the other twenty-one publications, UFOs were generally regarded as an unexplained phenomenon although some explanations were commonly expressed. These publications were actually mainly taken up with reports of UFO experiences.<sup>15</sup>

Groups of this type are usually called UFO research or investigation groups, and there would appear to be a large number of them spread throughout the world. The first documented civilian UFO research group was the Aerial Phenomena Research Organization, which was founded in the U.S.A. in 1952 and which had a membership of about 2,700 in 1979.<sup>16</sup> In Britain, dozens of small regional research groups have been formed and disbanded since the 1950s, and every major city probably harbours at least one: The Northern UFO Network listed twenty-one such groups in a 1980 directory. Three major national UFO research groups exist: The British UFO Research Association (BUFORA), formed in 1962, which had a membership of about 550 in 1981; Contact (UK), formed in 1967, and reported to have a membership of 1,000 in 1978; and the British UFO Society, formed in the mid-1970s, with a membership of about 600 in 1981.<sup>17</sup> It is these groups which consider themselves to be pursuing the subject of "ufology," and their members often refer to themselves as "ufologists."

Considering the number of people who belong to UFO research groups, both in this country and throughout the world, together with the controversial status of the "science" of ufology, it is surprising that there has been so little research into the membership of these groups. In the absence of such data, it is difficult to know how they might relate to the more familiar contactee cults. In order to answer this question and as part of a larger research project on the UFO movement in Britain, a questionnaire survey of members of the British UFO Research Association was conducted in 1981.

#### What kind of people join a UFO research group?

At the time of the survey (September 1981), there were approximately

550 members of BUFORA and 218 replies from Britain were received in time.<sup>18</sup> The survey findings will be presented under three headings: Firstly, social details; secondly, UFO and associated experiences and opinions; and lastly, religious beliefs.

#### Social details

Most of the respondents to the questionnaire were men (80%), and just over half (54%) were aged between 21 and 40 years. There were almost equal numbers of married and single people with 48% married, 43% single and 6% separated or divorced.

The formal education level was higher than the population average, with 12% of BUFORA respondents having a degree and 28% indicating that they had a professional qualification.<sup>19</sup> The informal education level was also high as the majority (66%) had engaged in some kind of further education whilst many of those who had not made such comments on the questionnaire, as:

"Self taught education - continuous"  
"Have consistently studied a variety of subjects all my life."  
"Most education obtained in later life."  
"Had to teach myself."

This seems to suggest that members consider it important to continue their education after full-time study had ceased, and further support for this inference would seem to come from the significance which they attach to reading. Most of them admit to reading one or more books a week (77%), and quite a few of these read three or more (13%).

A majority of respondents were in full-time employment (66%). While many different types of employment were represented, the most frequent was engineering (12%), with those in the civil service or local government second (9%), and journalists/writers, technicians and managers equal third in frequency with 5% each. Thirty percent of respondents were either self-employed or employers. Classifying occupations in accordance with categories used by the National Census, results in most being placed in socio-economic classes two, three and four; that is, what are generally known as the lower-middle and middle classes, as Table one shows.

It is clear from this that BUFORA members are very different from the social drop-outs associated with some UFO groups, such as the contactee cult studied by Balch and Taylor.<sup>20</sup> This is probably not surprising given the high subscription rate (£7.50 at the time of the survey and increased to £10 in 1982), coupled with the need for a fixed address in order to receive the publications produced by the society (BUFORA Journal/Bulletin and The Journal of Transient Aerial Phenomena).

Table One

The Socio-Economic Class Composition of BUFORA Members

Socio-economic class (from occupation)	Descriptive definition	% Members
1	Professional	5
2	Employers and managers	11
3	Intermediate and junior non-manual	34
4	Skilled manual (with own account non-professional)	17
5	Semi-skilled manual with personal service	4
6	Unskilled manual	2

The above scale is a collapsed version of that used by the Registrar General and is used in the General Household Survey. It is taken from I. Reid: op.cit., pp.38-39.

The large gender difference is probably due to the emphasis found in this type of group on the scientific nature of UFO research, given that the sex stereotypes in our society categorize science as a masculine activity.<sup>21</sup> This bias towards male membership is also found in other fringe science subjects: Loch Ness Monster enthusiasts and science fiction fans,<sup>22</sup> for example, although it should be noted that more men join voluntary organizations anyway, with the exception of religious groups.<sup>23</sup> Studies of contactee cults suggest that there may be a higher proportion of women involved in this type of group. Buckner states that the members of the group he studied were mainly elderly women, whilst Stuppel and McNeece found most well-established members of the Institute for Cosmic Research to be women, although the new members were predominantly young men and there were mainly men involved in building the flying saucer.<sup>24</sup>

A similar age distribution to BUFORA respondents is also apparent in fringe science subjects. Loch Ness Monster enthusiasts are mainly 21-35 years old and science fiction fans 18-35 years<sup>25</sup> although the reasons for this are not clear. In the case of science fiction fans it is certainly not because support has been drawn from one generation because the age distribution has remained fairly constant since the first surveys were carried out in the 1950s. It is a little more difficult to judge whether generational factors are involved in BUFORA membership because of a lack of comparable data. However, a survey of another UFO research group (Contact UK) carried out in the early 1970s, showed that 75% of members were aged between 21 and 40 years.<sup>26</sup> This could indicate that a generational factor was significant at that time and that the effect is declining as those concerned get older, but if this is the case then two generations are involved (those born in the 1930s and 1940s approximately). However, if those born in the 1930s are involved, then there should be

a substantial number of people in the 41-50 age range by now; but in fact only 13% of BUFORA respondents were of this age, only slightly more than those under 20 years (10%). There should also be less in the 21-30 range, but actually there are more (29% compared with 25% aged between 31 and 40). It does seem, therefore, that although the picture is confused, generational factors are probably not involved.

Research on the typical age distribution of members of voluntary associations is rather contradictory, with some pointing to 25-44, others seeing a peak at 40-50 and some studies seeing a generally high age distribution between 25 and 65.<sup>27</sup> Given such a wide age range, the most notable point about fringe science associations is that there would appear to be a drop in membership after about 40 years. It seems therefore, that these subjects are of particular interest to young men. The age distribution of contactee cults seems to vary considerably from group to group. Those in Buckner's study, for instance, were mainly elderly, unlike those in the group studied by Balch and Taylor, who were predominantly young.<sup>28</sup>

There would seem to be a high proportion of single people among BUFORA respondents. The survey showed that 43% of respondents were single, whilst the 1971 census for Great Britain indicated that 83% of males and 88% of females over 19 years were married, widowed or divorced. Qualifying the census figures is the fact that the manual/working classes tend to marry earlier whilst the majority of BUFORA respondents were both in the middle classes and in the young adult age range; so the 43% figure is probably not all that unusual. Nevertheless, the study of Loch Ness Monster enthusiasts also showed a high proportion of single people (50%) as did the one of science fiction fans (58%), although neither was considered unusual by the researchers due to the largely youthful membership. However, given the fact that research on voluntary associations indicates that more married people join than do single,<sup>29</sup> these figures might suggest that fringe science groups are not typical. One factor contributing to the slightly higher proportion of single people may be the largely male membership of this type of group, for if young males are not meeting members of the opposite sex in their principal leisure activity, then their chances of meeting suitable partners are necessarily decreased.

In view of the high proportion of respondents with professional qualifications and the considerable number who had engaged in further education, it appears that BUFORA members consider it important to continue their education beyond the normal school leaving age; and this conclusion seems to be supported by their extensive reading, but this of course would depend on the nature and level of the books being read. Some indication of this can be gathered from the subjects which they consider of special interest. Many respondents chose space research (60%) and astronomy (43%), which would indicate factual books. A considerable number also chose psychic research (49%) and science fiction (32%) whilst Fortean phenomena<sup>30</sup> (31%) was also popular. Although many books on psychic research and Fortean phenomena are not intended to be fiction in the same way that science fiction is; their status as fact is usually questioned. Often these books, and also many books on UFOs, are regarded as "faction" or "speculative fact." The type of book being read, therefore, is probably largely speculative and imaginative science, along with popular science,

particularly what might be called "frontier science" - subjects such as space research which deal with the exploration of new frontiers.

Research on cults has shown that members tend to have distinctive reading habits. Wallis, for instance, remarked that members of the Aetherius Society needed:

"...sufficient education to cope with the extensive occult literature and its abstruse terminology, but insufficient to penetrate its tortured logic and thin veneer of 'science'." <sup>31</sup>

Balch and Taylor refer to the fact that most studies show members of the cultic milieu to be avid readers, continually exploring through literature different metaphysical movements and philosophies and they comment:

"A significant part of their lives is devoted to the pursuit of intellectual growth, however undisciplined that may be in conventional academic terms." <sup>32</sup>

Unlike those in contactee cults, however, members of UFO research groups do not seem particularly interested in occult philosophy. Only 24% of BUFORA respondents indicated that this was one of their special interests, thus although there is a similar pattern of reading to these other groups, the content is not identical.

In sum, whilst further education would seem to be important to members of BUFORA, they would appear to be particularly interested in subjects which appeal to the imagination.

Finally, most respondents were of lower-middle or middle class status, and this is probably not too unexpected a finding given the predominantly middle class composition of voluntary associations in general. <sup>33</sup> But it is not clear whether the class composition is typical of other similar organizations. Unfortunately many studies of UFO groups do not give details of social class, or give only impressionistic data. Buckner judged those involved in the group he studied to be "upper working to lower middle class" and Balch and Taylor considered many members of the nomadic cult they studied to be "remnants of the counter culture" (which was largely middle class). <sup>34</sup> In the survey of subscribers to the Saucerian Press carried out by Stupple and Dashti, the majority of respondents were lower-middle class, but there were slight differences between those who read contactee books and those who read UFO research books by scientists such as Hynek. There was a tendency for the readers of UFO research books to be higher up the social scale than those interested in contactees. <sup>35</sup> Science fiction fans would also appear to be more middle class, as the professional class (also students) was over-represented in surveys carried out in Britain in 1954 and the U.S.A. in 1973; so also are Loch Ness Monster enthusiasts who are mainly in professional employment. <sup>36</sup>

No precise conclusions can be drawn from these studies, particularly as there are problems of comparability with social class data from Britain and the U.S.A., but there would appear to be a tendency for those in contactee cults to be slightly lower down the social scale (upper-manual/lower-middle) than those in UFO research groups (lower-middle/middle) and those seriously interested in science fiction (middle).

#### UFO and associated experiences and opinions

Those who join BUFORA have been interested in UFOs for a long time. The survey showed that 39% had been interested in UFOs from between five and fifteen years and 44% for over fifteen years. Studies show that belief in UFOs is more common among teenagers than adults, <sup>37</sup> and the survey suggested that most BUFORA members had been interested in UFOs since their teens. This is shown in Table two.

Table Two

The Length of Time BUFORA Members Have Been Interested In UFOs Compared With Their Age (in percentages)

<u>Age</u>	<u>Length of time interested in UFOs</u>			
	<u>Less than 1 year</u>	<u>1 - 5 years</u>	<u>5 - 15 years</u>	<u>15 plus years</u>
Under 20	9	46	41	4
21 - 30	2	25	55	18
31 - 40	0	9	43	48
41 - 50	0	3	28	69
51 - 60	0	4	14	82
over 61	0	0	29	71

When asked how they became interested in UFOs, a large proportion of the respondents indicated that a particular book had sparked their interest (35%), others said that it had been a news event such as a report of a UFO sighting (21%), whilst 19% said that personal experience of a UFO had been the trigger.

Most members (61%) claimed to have seen something which they considered to be a UFO, but the majority involved nothing more than lights in the sky (48%) whilst 6% indicated that they had experienced a close encounter and less than one percent a contact with aliens. A few (5%) claimed more than one type of experience.

Although a large proportion of members reported having had a psychic experience of some kind (57%), the two types of experience do not appear to be strongly correlated as those who reported seeing a UFO were only slightly more likely to report having had a psychic experience, than those who did not. Even though comparable figures for the general populations are difficult to find, the incidence of these experiences among BUFORA members seems high. For instance, a poll in the U.S.A. in 1973 found that 11% of the population claimed to have seen a UFO. <sup>38</sup> The high figure for psychic experience may be less unusual as it is similar to that found in a survey of the San Francisco Bay Area in 1973. <sup>39</sup>

The view of UFO group members commonly held by the general public is that they believe UFOs to be extra-terrestrial spacecraft. In fact, although a large proportion of respondents (44%) gave this as their



first answer when asked what UFOs might be, and another 11% chose it for second or third place, many did not select this possibility at all. Actually two other ideas about what UFOs might be featured quite prominently these were that they could be an unknown kind of natural phenomena (19% put it first and another 29% placed it second or third) and that they could be psychic events (12% chose this first and another 23% put it in second or third place). Also, in contradiction to popular expectations, the idea that they were only misperceptions drew support from a sizeable minority of members (23% placed it as one of their first three choices). The least popular suggestions were that UFOs were spiritual beings, secret weapons, or evil entities. Among the other possibilities suggested were holograms and time travellers, whilst 8% said they had no idea what UFOs might be.

It is clear from the survey that BUFORA members do not uncritically accept all that is contained in the UFO literature. Most agreed<sup>40</sup> that "when man finally solves the UFO mystery, it will cause a revolution in his current understanding of the world" (71%), but although popular books and films often suggest that "the governments of the world are in contact with aliens and are withholding this information from the general public" only 23% of respondents agreed with this statement.

Despite the fact that the majority of respondents thought UFOs were likely to be extra-terrestrial vehicles, they were divided over whether "aliens are living on the Earth and mixing with human beings" with 38% in agreement, 38% unsure and 24% in disagreement. However, a majority (55%) considered that some ufologists have been visited by "Men in Black," but then they are frequently considered to be government agents or psychic manifestations rather than alien beings. The idea that "cattle mutilations are associated with UFOs" has been expressed in a number of popular books and articles, but BUFORA respondents were uncertain about this connection with nearly half choosing the "unsure" category.

Erich von Däniken's idea that "man was created by the genetic engineering of extra-terrestrials who visited Earth millions of years ago" helps to sell a large number of books, but respondents were divided over this suggestion with 31% in agreement, 39% unsure, and 27% in disagreement. Also popular with the general public is the idea that "man once inhabited a lost continent called Atlantis where he had knowledge and powers that he does not now possess," but this also failed to gain widespread agreement among respondents with 39% in agreement, 31% unsure, and 30% in disagreement.

Given the lack of support (but familiarity with) the last two themes expressed in books associated with UFOs, it seems possible that the popularity of these books could be due less to the central "theory" than to the additional material they contain. In the case of von Däniken, this consists of evidence which is supposedly unexplained by science and thus is still a mystery, whereas books on Atlantis present a supposedly lost knowledge or occult philosophy. The reverse is suggested by Ashworth who considers the themes to be of central importance.<sup>41</sup> He views "Danikenism" and "Atlantism" as two distinct modern myths which are structurally related to Judaeo-Christian millenarianism and Greek Materialism on the one hand (Danikenism) and to Platonic Eleaticism on the other (Atlantism). He suggests that books written by writers on Atlantis, such

as John Mitchell:

"...have the same status with Atlanticists as Daniken's Chariot of the Gods (sic) has with flying saucer buffs, or Danikenist."<sup>42</sup>

From the survey it would seem that neither myth has much status with members of BUFORA. As for the Daniken myth being more popular among "flying saucer buffs," more BUFORA respondents agreed with the Atlantis myth! These two new myths may be structurally distinct as Ashworth suggests, but this does not mean that individuals must of necessity believe either one or the other. In fact, 50% of those who strongly agreed with the Daniken myth also strongly agreed with the Atlantis myth.

What seems to be important, at least to BUFORA members, is not the new myth but the fact that they consider the evidence to be anomalous; that is, unexplained by science or religion. Ashworth acknowledges the concern with anomalies in these books (although he seems to view them as genuine anomalies rather than socially constructed ones<sup>43</sup>) but considers the new explanation of the anomalies to be more important than the anomalies themselves. An additional explanation for an interest in these new myths might be the existence of a world view in which mysteries are the central element.

This idea gains some support from the BUFORA survey results which indicate that although those in UFO research groups are interested in UFOs and other similar subjects, they are often in disagreement over opinions and beliefs - but most of them agree that UFOs are a mystery. What they have in common, then, is a similar interest in the mysterious nature of UFOs.

#### Religious beliefs

The popularly expressed view that UFO groups are largely religious phenomena caused respondents to be particularly sensitive about the topic of their religious beliefs. This was because one of the stated aims of the organization is "to encourage, promote and conduct unbiased scientific research of unidentified flying objects (UFO) phenomena throughout the United Kingdom" and thus they consider their interest to be scientific rather than religious.<sup>44</sup> Although most answered the relevant survey questions, a few added comments expressing their failure to see the connection between the questions and their interest in UFOs. The extent to which this may have affected responses to these questions is not known, but it needs to be taken into consideration during the interpretation of the results.

The survey showed that those in BUFORA were more likely than the general population to break with traditional religion. Although slightly more members considered considered that they belonged to a religion than did not, 40% indicated that they were not affiliated to a church whereas only 8% of the general population fall into this category.<sup>45</sup> Also the percentage of members who never attended church (58%) was higher than that for the general public (27%). However, they are not more likely to join unorthodox religious groups; few indicated categories such as "Eastern" or "Witchcraft/Pagan."

It might be thought that the low percentage of those affiliated to a church reflects a concern to underplay any possible connection between UFO research and religion, for reasons mentioned above, but this seems unlikely because the low church membership is not matched by a low incidence of belief in God. The majority (61%) said they believed in God, and this figure is similar to that for belief in God among the general population (68% for men), as is the figure for members of BUFORA who are nonbelievers (17%) when compared with the general population (20% for men).<sup>46</sup>

A particularly interesting finding is that most respondents indicated that they believed in life after death (68%), and just under half believed in reincarnation (45%), and these percentages are much higher than those for the general population where they are 35% for life after death and 28% for reincarnation.<sup>47</sup> One reason for this higher percentage might be that members subscribe to mystical rather than traditional religious beliefs. About 45% believed in an impersonal spirit or life force, which is not dissimilar to the figure for the general population (41%)<sup>48</sup>; but another element of mysticism (besides the tendency to believe in reincarnation) is the notion that each human being is travelling a path of spiritual evolution which will end in union with God,<sup>49</sup> and 41% of members agreed with this statement.

Studies of contactee cults have referred to the mystical beliefs of their members,<sup>50</sup> so this could be seen as support for the view that UFO research groups are essentially similar to religious cults, but this would be an over-simplification for two main reasons: Firstly, although a large proportion of BUFORA respondents appeared to have mystical rather than orthodox religious beliefs, the majority did not; and, secondly, it is not known how common mystical beliefs are among the general population, although it has been suggested that mystical religion is prevalent among educated people.<sup>51</sup>

### Conclusion

The survey implies that those in UFO research groups are very different from those in contactee cults, although there are some interesting similarities. For instance, reading seems to be important to those in both types of groups, but although there may be some overlap in the literature read, members of research groups seem to be more interested in imaginative science than in occult philosophy. Their involvement in the subject of UFOs would appear to be one aspect of a more general interest in what they consider to be the "frontiers of science"; subjects such as space research, astronomy and psychic research.

Their interest in science subjects such as astronomy, draws attention to similarities between members of UFO research groups and those who practice amateur science. Like amateur scientists they are "leisure time entrepreneurs."<sup>52</sup> That is, they share a similar seriousness with regard to their leisure in contrast to the majority of people who participate in popular leisure which is primarily nonserious. Unlike amateur scientists, however, they seem to be particularly drawn towards "anomalies," that is, subjects which they consider to be unexplained by science.<sup>53</sup> Why is this?

One explanation could be that they are more interested in mysteries than in their solution. Those in contactee cults are also interested in mysteries, but they are concerned with metaphysical or occult knowledge and experiences, that is, sacred mysteries, rather than anomalies. Nevertheless, UFOs, the Bermuda Triangle, poltergeists and the like, are portrayed in popular literature as ambiguous; they exude charisma and are pregnant with potential meaning.

Does this mean that, like members of contactee cults, they have mystical beliefs? The lack of support for institutional religion coupled with the high occurrence of belief in reincarnation, an impersonal spirit or life force, and spiritual evolution, might indicate that some of them do; but there is no clear indication that these beliefs go together to form a coherent mystical pattern as they do for those in cults. Members of UFO research groups could share a similar "epistemological individualism"<sup>54</sup> to those who have mystical beliefs without sharing all of these beliefs. In fact, a different pattern of beliefs could be involved, and as the study by Emmons and Sobel suggests,<sup>55</sup> there may be a collection of nonreligious paranormal beliefs (involving UFOs, the Loch Ness Monster, ESP, Sasquatch, etc.) which are a functional alternative to mainstream religion.

There could, however, be other explanations for the interest in mysteries and anomalies. Topics such as UFOs are often portrayed in popular literature as constituting a "challenge to science,"<sup>56</sup> and so the important factor may be an apparent threat to science rather than the existence of a mystery as such. This interpretation is suggested by a number of writers. Sheaffer, for instance, considers ufology to be a social movement which is "fundamentally a reaction against science and reason,"<sup>57</sup> and part of a larger revival of interest in the occult. Others, such as Cotgrove,<sup>58</sup> identified a growing anti-science movement in the early 1970s that included intellectuals as well as the proliferation of cults, and which he suggested was associated with a particular anti-authoritarian personality type valuing imagination, experience, spontaneity and community.

Nowotny,<sup>59</sup> however, considers attacks on science to be less a rejection of science than a dispute over who is to control a future world and what it is to look like. She makes a distinction between anti-science and the pseudo-sciences, seeing the latter as a challenge to the monopoly of science on definitions of nature and the like, rather than a disagreement with science over how subjective knowledge should be verified, as in the case of anti-science.

A further explanation for the interest in anomalies might be that those involved are largely ignorant of the requirements of scientific research. After all, most members of UFO research groups (and other groups dedicated to research on anomalies<sup>60</sup>) are nonscientists; and, as Snow and Machalek<sup>61</sup> point out, belief is more "natural" than disbelief. The scientific attitude, in which common sense assumptions are questioned, has to be learnt.

Not only that, but popular views of science are frequently misconceptions. Holton<sup>62</sup> describes seven of these popular misconceptions, one of which-- the image of science as magic-- seems particularly appropriate to ufologists in the light of their interest in space research and science

fiction. In this image, it appears that science knows no inherent limitations. If such is the case, of course, there is nothing which might be impossible to scientifically advanced extra-terrestrials, and nothing which might not be possible on Earth, either.

It seems likely that all of these explanations are involved and that ufology is a subject which contains conflicting approaches, making each of the categories of "pseudo-science," "anti-science" and "developing science" partially true. The particular way in which these different approaches interact and the tensions produced by such diversity, appear to be important. Blake,<sup>63</sup> for instance, suggests that ufology is a developing science because the two main ways of accounting for UFOs, to include them within the bounds of normal science or to present them as something beyond the confines of normal science, are coalescing. In Britain, however, there is a considerable amount of conflict between these factions, and it seems just as possible that they will split apart altogether.<sup>64</sup>

Finally, although the intention of this article has been primarily to present some details about members of a UFO research group, rather than to discuss the wider implications of the findings, some preliminary observations can be made. As far as the UFO movement is concerned, the study of UFO research groups shows that like many social movements, it consists of a number of different types of movement organizations.<sup>65</sup> At the very least, there are contactee cults and research groups. Apart from the obvious need to study the different types of organizations and their inter-relationships if the nature of the UFO movement is to be understood, there are other implications.

UFO groups are part of a wider subculture which is usually referred to as the cultic milieu.<sup>66</sup> This is described as having a religion-science axis upon which cults can be placed,<sup>67</sup> but do these two poles also represent diverging tendencies within cult movements as they appear to in the UFO movement (and also astrology<sup>68</sup>)?

Also, is the same kind of religion and science involved in all cases? A number of writers have identified the religion as mysticism but others have described similar beliefs as "metaphysical," "esoteric" or "the alternative reality tradition,"<sup>69</sup> and it is not clear whether they are all equatable. Certainly, as far as UFO research groups are concerned, it does not appear to be a straightforward case of mystical religion. The science is usually termed "pseudo-science" or "anti-science," but as those writing with the philosophy and sociology of science have shown,<sup>70</sup> such terms are problematic. It is important to examine the practices, beliefs and attitudes towards science of those involved in "pseudo-sciences" like ufology in order to find out why they are described in this way. The survey suggests that in some cases an interest in mysteries could be an important factor which might be a link to mysticism. On the other hand, an ignorance of scientific method or a mistaken view of science could be involved. In addition, it would be wrong to ignore the practices and attitudes of those who apply categories such as a "pseudo-science" because the use of these terms results in particular consequences for those interested in subjects like ufology.

In sum, information about UFO research groups is of particular importance to research on the cultic milieu. More generally it is of interest to

the sociology of science, especially to analyses of "pseudo-science," and it also provides useful material for research on social movements and voluntary associations.

#### Footnotes

<sup>1</sup>Although I am only concerned here with sociological and social psychological research, other types of research on the topic of UFOs has been carried out. For instance, psychological: D.G. Jung, *Flying Saucers*, Routledge and Kegan Paul, London, 1959; R. Haines: *Observing UFOs*, Nelson-Hall, Chicago, 1980; R. Haines (ed), *UFO Phenomena and the Behavioral Scientist*, Scarecrow Press, Metuchen, N.J., 1979, historical: D. Jacobs, *The UFO Controversy in America*, Indiana University Press, Bloomington, 1975; bibliographical: L. Catoe, *Bibliography on UFOs and Related Subjects*, Gale, U.S.A., 1979. For more general information, see: C. Sagan and T. Page (eds), *UFOs-A Scientific Debate*, Cornell University Press, Ithaca, 1972; A. Hendry, *The UFO Handbook*, Sphere Books, London, 1980; R.D. Story (ed), *The Encyclopedia of UFOs*, New English Library, London, 1980.

<sup>2</sup>D. Warren, "Status Inconsistency Theory and Flying Saucer Sightings," *Science*, Vol. 170, 1970, 599-603. Information about his subsequent studies was obtained from: R. Westrum, "Social Intelligence about Anomalies: The case of UFOs," *Social Studies of Science*, Vol. 7, 1977, 271-302.

<sup>3</sup>Westrum: *op. cit.*, p.278.

<sup>4</sup>S. Resta, *The Relationship of Anomie and Externality to Strength of Belief in UFOs*, MA thesis, Loyola College, U.S.A., 1975.

<sup>5</sup>L.W. Littig, "Affiliation Motivation and Belief in Extra Terrestrial UFOs," *Journal of Social Psychology*, Vol. 83, 1971, 307-308.

<sup>6</sup>P. Fox, "Social and Cultural Factors Influencing Beliefs about UFOs," in R. Haines (ed), *op. cit.*, p.23.

<sup>7</sup>See for instance; R.S. Ellwood, *Religious and Spiritual Groups in Modern America*, Prentice-Hall, N.J., 1973; C. Glock and R. Stark, *Religion and Society in Tension*, Rand McNally and Co., 1973.

<sup>8</sup>L. Festinger, H. Riecken and S. Schachter, *When Prophecy Fails*, Harper Torchbooks, New York, 1964 (1956); J.A. Jackson, "Two Contemporary Cults," *The Advancement of Science*, Vol. 23, No. 108, 1966, 60-64. H. Taylor Buckner, "The Flying Saucerians," *New Society*, September 9, 1965; R. Wallis, "The Aetherius Society: A Case Study in the Formation of a Mystagogic Congregation," *Sociological Review*, Vol. 22, 1974, 27-44; R. Balch and D. Taylor, "Seekers and Saucers," *American Behavioral Scientist*, Vol. 20, No. 6, 1977, 839-860. D. Stupple and W. McNeese, "Contactees, Cults and Culture," *Mutual UFO Network (MUFON) Symposium Proceedings*, U.S.A., 1979.

<sup>9</sup>R. Wallis, *op. cit.*; also, R. Wallis, "Ideology, Authority and the Development of Cultic Movements," *Social Research*, Vol. 41, No. 2, 1974, 299-327.

<sup>10</sup>See for example, R.S. Ellwood, *op. cit.*, p.135.

<sup>11</sup>M. Schutz, *Organizational Goals and Support Seeking Behavior: A Comparative Study of Social Movement Organization in the UFO (Flying Saucer) Field*, Ph.D.dissertation, Sociology Department, Northwestern

University, 1973. A short summary of this work is contained in; R. Story (ed), op. cit., pp. 340-341.

<sup>12</sup>D. Stupple and A. Dashti, "Flying Saucers and Multiple Realities," Journal of Popular Culture, Vol. 11, 1979, 479-493.

<sup>13</sup>J. Blake, "Ufology: The Intellectual Development and Social Context of the Study of UFOs," in R. Wallis (ed), On the Margins of Science, Sociological Review Monograph, 27, Keele University, Staffs., 1979.

<sup>14</sup>The UFO Directory, advertised in a number of issues of Flying Saucer Review during 1981-82, claimed to be a "comprehensive world guide to UFO organizations, groups and publications, book suppliers, photo and cassette suppliers, and UFO News-clipping services." It was supplied by the UFO Network, 39, Birkbeck Road, London, NW7. In order to overcome postage problems and limit the material, only publications produced by British organizations which were interested in UFOs, were used. These were: Aetherius Society Newsletter; Awareness; Axminster Light Centre Newsletter; Bufora Journal; Earthlink; Flying Saucer Review; Foresight; Helios News; Interplanetary News; Irish UFO News; Investigation; Journal of Transient Aerial Phenomena; Kingdom Voice; Lantern; Magonia; Mersey News; Northern UFO News; Occult World; Pegasus; Quest (UFO); (Mapit) Sky-watch; Skywatch Gazette; The Atlantean; The Probe Report: UFO Insight; UFO News Bulletin; UFO Research Review: UFOs-A Serious Study; Viewpoint Aquarius. Not all of these publications were given in the UFO Directory; a few were obtained by following up advertisements in the publications mentioned by the Directory.

<sup>15</sup>The term "UFO experience" is used to describe an experience which is said to involve interaction with a UFO. It is taken from, J.A. Hynek, The UFO Experience: A Scientific Inquiry, Corgi Books, London, 1978, (1972).

<sup>16</sup>See R. Story (ed), op. cit., p. 4-5. UFO research was actually begun by the American Air Force at the end of 1947 after 156 reports had been received by them. Project Sign, as the initial research body was designated, was changed to Project Grudge in February 1949 and concluded in August 1949, that: "...all evidence and analysis indicated that UFOs were the result of misinterpretation of various conventional objects...." or "...a mild form of mass hysteria and war nerves, according to D. Jacobs, op. cit., p. 49. Civilian amateur research groups were formed by people who were not satisfied with the official explanation.

<sup>17</sup>Sources of these figures are: A personal communication with the Director of Publications of BUFORA at the time of the survey; The Yorkshire Post, October 30, 1978, for Contact (UK); and an interview with the President of the British UFO Society, during 1981.

<sup>18</sup>Copies of the questionnaire went out with the September issue of BUFORA's bimonthly magazine, originally called Bufora Journal, but subsequently altered in format due to financial problems and re-named BUFORA Bulletin. Postage on the questionnaires was pre-paid. Comparison with the complete membership on the one variable for which figures were available (gender), shows that 20% of respondents were female and 19% of the total membership were female, indicating that at least as far as this variable is concerned, those who answered the questionnaire were a representative sample.

<sup>19</sup>It seems likely, though, that those with qualifications were over-represented among respondents because of the requirements of completing

forms and questionnaires.

<sup>20</sup>R. Balch and D. Taylor, op. cit., p.849, who write: "Other things being equal, a man with a good job, a family, and a respectable position in the community is less likely to join a flying saucer cult than a single male living alone or in a commune, with few material possessions and a strong penchant for change and excitement."

<sup>21</sup>H. Weinreich-Haste, "What Sex is Science?" in O. Hartnett, G. Boden and M. Fuller (eds), Sex-Role Stereotyping, Tavistock Pub., London, 1979.

<sup>22</sup>R. Grimshaw and P. Lester, "Surveying Monster Enthusiasts," Birmingham Centre for Contemporary Cultural Studies, 1981 (Mimeographed). A. Berger, "Science Fiction Fans in Socioeconomic Perspective," Science Fiction Studies, Vol. 4, 1977, p. 232-246.

<sup>23</sup>I. Reid, Social Class Differences in Britain, Open Books Pub., London, 1977, p. 220.

<sup>24</sup>H. Taylor Buckner, op. cit.; D. Stupple and W. McNeece, op. cit.

<sup>25</sup>R. Grimshaw and P. Lester, op. cit.; A. Berger, op. cit.

<sup>26</sup>This information was given by letter from the Senior Research Officer of Contact (UK), Data Research Division, who said that the survey report had been mislaid. The information was obtained before my own survey was carried out.

<sup>27</sup>C. Smith and A. Freedman, Voluntary Associations: Perspectives on the Literature, Harvard University Press, Cambridge, Mass., 1972; M. Hausknecht, The Joiners, Bedminster Press, New York, 1962.

<sup>28</sup>H. Taylor Buckner, op. cit.; R. Balch and D. Taylor, op. cit.

<sup>29</sup>C. Smith and A. Freedman, op. cit., p. 116.

<sup>30</sup>The term Fortean phenomena, refers to supposedly unexplained natural events such as spontaneous human combustion, falls of fish, frogs and the like, coincidences, psychic phenomena and other "strange phenomena." The term derives from Charles Fort (1874 - 1932), who collected accounts of these types of events which he claimed were "excluded by science." See C. Fort, The Book of the Damned, Sphere Books Ltd., London, 1973 (1919), and the magazine Fortean Times, BM-Fortean Times, London, WC1N 3XX.

<sup>31</sup>R. Wallis, op. cit. Sociological Review, p. 38.

<sup>32</sup>R. Balch and D. Taylor, op. cit., p. 850.

<sup>33</sup>

C. Smith and A. Freedman, op. cit.; I. Reid, op. cit.

<sup>34</sup>H. Taylor Buckner, op. cit.; R. Balch and D. Taylor, op. cit.

<sup>35</sup>D. Stupple and A. Dashti, op. cit. They statistically separated those who recommended books written by contactees from those who recommended books written by UFO researchers. Only 13% of those who recommended contactee books were in the upper middle class, whereas 22% of those who recommended research books were. Conversely, 28% of those who recommended contactees were in the upper lower class and only 17% of those who recommended scientific researchers.

<sup>36</sup>A. Berger, op. cit.; 31% were in the professional class in a 1954 study and 37% in a study in 1973, compared with 24% of the total population in that class in 1973 (U.S.A. data); R. Grimshaw and P. Lester, op. cit.;

Ten were professionals and two quasi-professionals (e.g., police, nurses) out of the twenty-five respondents in this small survey.

<sup>37</sup>B. Martin and R. Pluck, "Young People's Beliefs," General Synod Board of Education, 1977; "National Survey of Religious Attitudes of Young People," sponsored by Buzz Magazine, 1978.

<sup>38</sup>Gallup Poll news release, November 29, 1973, cited in R. Westrum, op. cit., p. 277.

<sup>39</sup>C. Glock and R. Wuthnow, "Departures from Conventional Religion: The Nominally Religious, the Nonreligious and the Alternatively Religious," in R. Wuthnow (ed), The Religious Dimension, Academic Press, London, 1979. The survey, conducted in 1973, found that 50% of those under 30 years old and 45% of those over 30 years, reported having had ESP experiences.

<sup>40</sup>For the purpose of this analysis of popular themes in UFO literature, the survey categories of "strongly agree" and "agree" have been collapsed to indicate agreement, and those of "strongly disagree" and "disagree," to form disagreement.

<sup>41</sup>C.E. Ashworth, "Flying Saucers, Spoon Bending and Atlantis: A Structural Analysis of New Mythologies," Sociological Review, Vol. 28, No. 2, 1980, 353-376. Oddly, Ashworth does not mention a very similar analysis carried out by M. Carroll, "Of Atlantis and Ancient Astronauts: A Structural Study of Two Modern Myths," Journal of Popular Culture, Vol. 11, No. 3, 1977, 541-550.

<sup>42</sup>C.E. Ashworth, op. cit., p.363.

<sup>43</sup>Contrary to Ashworth's suggestion, both science and religion do actually offer explanations for subjects such as UFOs, psychic phenomena, the Bermuda Triangle and the Turin Shroud, but those who read popular books on these subjects are not satisfied with the explanations offered and so consider them to be unexplained or anomalous. Although Ashworth's analysis is interesting, there are a number of problems with it. Two are particularly important: Firstly, he leaps in an unproblematic manner from the writers of the books under consideration to their audience. Since his article is about themes in books, he can only speculate about what they mean to those who read them. This failure to adequately distinguish between books and their readers is present right from the beginning where he fails to consider the possibility that magazines such as UFO, Prediction, Alpha and Omni, might have different audiences. Not only that but editors may deliberately aim at a wide variety of audiences in order to be successful. Secondly, ashworth's analysis ignores social conflict; that is, the existence of social groups with different interests. He mentions the "alternative" character of the books in question, but his reason for why these new myths are popular is explained totally at the level of ideas: that they resolve contradictions which are no longer resolved by science and religion. The fact that these books are usually read by a non-academic audience and are usually considered to challenge science, is given no real consideration. Why, for instance, did books on "the unexplained" become particularly popular with the emergence of the counter-culture in the mid-1960s?

<sup>44</sup>However, many of those in UFO research groups appear to have a complicated and ambiguous attitude towards science. Unfortunately, only one question relating to science was asked in the survey, and this was taken from a survey of European values being conducted from the Survey Research Unit, North London Polytechnic. It was the rather problematically-worded

question: "Do you think that science will eventually solve all of life's mysteries or are there some things in life which science will never be able to explain?" a not too dissimilar proportion of respondents chose "will never be able to explain it all" (71%), as did those in the pilot study of the European Values Survey (84%) (the question was omitted from the main study). However, the slightly higher proportion of those favourable towards science among ufologists, might be of more importance than the small statistical difference would suggest. Interviews and open-ended questionnaires suggest that members of UFO research groups are pro-science, but that a number of conflicting popular images of science are involved in their views. For instance, on the one hand, science as unimaginative and limited, and on the other, science as the producer of exciting technology.

<sup>45</sup>Gallup Poll, C0662, 21 - 26 March, 1979.

<sup>46</sup>Ibid. The slightly lower percentages among my respondents on both belief and non-belief in God may be due to an additional category of "sometimes" included in the choices on my questionnaire. This drew 14% of respondents. Of course, some may have chosen this category in preference to "don't know," which drew only 6% compared with 9% of the general population who chose it.

<sup>47</sup>Percentages for life after death are taken from the Gallup Poll conducted in June 1975, and for reincarnation from the Gallup Poll which took place in March, 1979.

<sup>48</sup>Gallup Poll of March 1979, op. cit.

<sup>49</sup>E. Troeltsch, The Social Teaching of the Christian Churches, Vol.2, Halley Stewart Pub., 1931, 735-736.

<sup>50</sup>Actually they refer to the fact that members are believers in the occult and psychic and that they are metaphysical seekers, but many of the beliefs described are similar to those involved in mystical religion. Unfortunately, when describing what appears to be the same social phenomena, different authors have used a variety of terms, such as "alternative reality tradition," "esotericism," "metaphysical world view" and "mystical religion," and it is not yet clear whether they are all comparable. See, R.S. Ellwood, op. cit.; E. Tiryakian, "Toward the Sociology of Esoteric Culture" in E. Tiryakian (ed), On the Margin of the Visible, John Wiley and Sons, New York, 1974; R. Balch and D. Taylor, op. cit.; C. Campbell, "The Secret Religion of the Educated Classes," Sociological Analysis, Vol. 39, No. 2, 1978, 146-156; W.H. Swatos, "Church-Sect and Cult: Bringing Mysticism Back In," Sociological Analysis, Vol.42, No.1, 1981, 17-26.

<sup>51</sup>C. Campbell, op. cit., 1978

<sup>52</sup>R. Stebbins, "Avocational Science: The Amateur in Archaeology and Astronomy," International Journal of Comparative Sociology, Vol.21, No. 1-2, 1980, 34-48.

<sup>53</sup>The term "anomaly" is becoming popular as an alternative (and more general) term to "paranormal." For example, a splinter group from the Society for Psychic Research was formed in 1981, and calls itself "The Association for the Scientific Study of Anomalous Phenomena." See also M. Truzzi, "On Pseudo Sciences and Proto Sciences," The Zetetic, Vol. 1, No. 2, 1977, 3-8 (editorial).

<sup>54</sup>R. Wallis, op. cit., Social Research.

<sup>55</sup> C.F. Emmons and J. Sobel, "Paranormal Beliefs: Functional Alternatives to Mainstream Religion?" Review of Religious Research, Vol. 22, No. 4, 1981, 301-312.

<sup>56</sup> For example, J. Vallee and J. Vallee, Challenge to Science - The UFO Enigma, Henry Regnery, Chicago, 1966.

<sup>57</sup> R. Sheaffer, The UFO Verdict, Prometheus Books, Buffalo, New York, 1981, p. 237.

<sup>58</sup> S. Cotgrove, "Anti-science," New Scientist, July 12, 1973, p. 82-84.

<sup>59</sup> H. Nowotny, "Science and its Critics: Reflections on Anti-science," in H. Nowotny and H. Rose (eds), Counter Movements in the Sciences, Reidel, Dordrecht, 1979.

<sup>60</sup> Although a number of scientists are involved in research on these subjects, particularly as independent researchers or as members of large well-established organizations such as the Society for Psychic Research in Britain or the Centre for UFO Studies in the U.S.A., the majority are not scientists.

<sup>61</sup> D.A. Snow and R. Machalek, "Second Thoughts on the Presumed Fragility of Unconventional Beliefs," paper given at the 1981 Sociology of Religion Conference, Lincoln University.

<sup>62</sup> G. Holton, "Modern Science and the Intellectual Tradition," Science, Vol. 131, 1960, 1187-1193.

<sup>63</sup> J. Blake, *op. cit.*

<sup>64</sup> This conflict can be seen in the editorial and letters sections of publications produced by UFO research organizations. It often appears to be characterised as the "nuts and bolts" approach versus the "paranormal" approach, as illustrated in the following letter from (Mapit) Skywatch, No. 40, Sept/Oct., 1981:

"I was interested to read the latest installment in the 'nuts and bolts paranormalists' saga, which seems to be demanding a higher platform than it warrants....If we are to limit our studies to 'unexplained things seen within the context of flight' then are we to ignore reports of landed craft or apparent entities? Additionally, if a person, or persons, psychically visualises a UFO (possibly with occupants) that is not physically there by any scientific standards, then doesn't that alone need investigation?"

<sup>65</sup> J. McCarthy and M. Zald, "Resource Mobilization and Social Movements: A Partial Theory," American Journal of Sociology, Vol. 82, 1977, 1212-1241.

<sup>66</sup> C. Campbell, "The Cult, the Cultic Milieu and Secularisation," A Sociological Yearbook of Religion in Britain, 5, 1972.

<sup>67</sup> *Ibid.*

<sup>68</sup> D. Newton, "A Sociologist Examines Contemporary Organized Astrology," Astrology, Vol. 55, No. 4, 1981, 129-135; Vol. 56, No. 1, 1982, 7-16.

<sup>69</sup> The term "metaphysical" has been used by R. Balch and D. Taylor, *op. cit.*; that of "esoteric" by E. Tiryakian, *op. cit.*; and "the alternative reality tradition" by R.S. Ellwood, *op. cit.*

<sup>70</sup> In the philosophy of science there is the debate between theorists such as K. Popper, T. Kuhn, I. Lakatos, and P. Feyerabend. In the sociology

of science, there are writers such as B. Barnes and M. Mulkey. Of particular interest are: H. Nowotny and H. Rose (eds), *op. cit.*; and M. Hanen, M. Osler, and R. Weyant (eds), Science, Pseudo Science and Society, Wilfred Laurier University Press, Waterloo, Ontario, 1980.



# CHINESE PARAPSYCHOLOGY

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COMPILED BY MARCELLO TRUZZI

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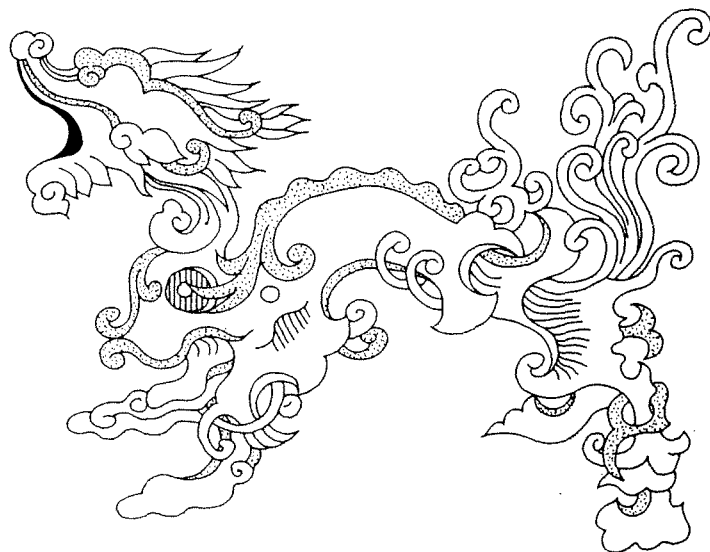
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## HISTORICAL NOTES ON A SEANCE WITH EUSAPIA PALLADINO IN 1912

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One of the best known physical mediums in the history of parapsychology was the Italian Eusapia Palladino (1854-1918).<sup>2,3</sup> Many renowned psychical researchers of the past -- Aksakof (Rapport, 1893), Bozzano (1903), Carrington (1954; Feilding, Baggally and Carrington, 1909), Flammarion (1897, 1907), Flournoy (1911), Hodgson (H. Sidgwick, 1895), Lodge (1894, 1895), Lombroso (1909), Myers (1894, 1895), Ochrowicz (1896; Krauz, 1894), Richet (1893, 1895), Rochas (1897, 1898, 1906), Schrenck-Notzing (1920), H. and E.M. Sidgwick (E.M. Sidgwick, 1894; H. Sidgwick, 1895), Warcollier (1958) and others<sup>4</sup> -- had sittings with her in which they studied her purported telekinetic and materialization phenomena (for overviews of these investigations see: Alippi, 1962; Carrington, 1909; Dingwall, 1950; Fodor, 1933; Inglis, 1977; Nicol, 1956; Rochas, 1906; and Tietze, 1972). Although she was detected in fraudulent activities several times (e.g., Courtier, 1908, pp. 523-525; Feilding et al, 1909, p. 383; Flammarion, 1907, pp. 110, 203; Krebs, 1910; Lombroso, 1909, pp. 102, 306),<sup>5</sup> many of her investigators were convinced of the genuineness of at least some of her phenomena. Others, however, remained unconvinced of the paranormal nature of any of Eusapia's phenomena (e.g., Finch, 1909; Podmore, 1911; E.M. Sidgwick, 1909).

Eusapia's mediumship was of great assistance to the development of physical mediumship studies. In J. Fraser Nicol's (1956) opinion, Eusapia's positive disposition to be investigated "permitted the development of rigorous methods of control and of exact recording of facts at the moment of their occurrence to a degree wholly unknown before her time." (p. 33) Examples of this may be found in the reports of Fontenay (1908), The Institut Général Psychologique (Courtier, 1908), and in the 1908 S.P.R. investigation in Naples (Feilding et al, 1909).

Other methodological advances in the study of physical mediumship partly developed from, or influenced by the investigation of Eusapia's phenomena were the use of instruments to test and record physical phenomena<sup>6</sup> (e.g., Aggazzotti, Foà, Foà and Herlitzka, 1907; Bottazzi, 1907; Courtier, 1908; Imoda, 1908; and Lombroso, 1909), and the observation and analysis of psychological, psychiatric and physiological aspects of mediumship (e.g., Courtier, 1908; Favre, 1910; Krauz, 1894; Lombroso, 1892, 1909; Marzorati, 1909; and Morselli, 1908).

Eusapia's case is considered so important as evidence for paranormal physical phenomena that her mediumship still is discussed in modern publications both of skeptics (e.g., Hansel, 1980; Rawcliffe,

1959) and believers in her phenomena (e.g., Rogo, 1978; Tocquet, 1972). Richet (1923) considered her case so important that he wrote: "Even if there were no other medium than Eusapia in the world, her manifestations would suffice to establish scientifically the reality of telekinesis and ectoplasmic forms." (p. 34)

Since the prominence and importance of Eusapia's case justifies the study of obscure aspects of her mediumistic career, this paper will present a report of a 1912 séance never published in English that extends our knowledge of Eusapia's activities to a period where little is known about her.

## II

There is little information in English of Eusapia's mediumistic career after 1910 (Alvarado, 1982). Eusapia gave séances in New York from November 1909 to June 1910, where her performances caused considerable publicity and controversies (e.g., Carrington, 1954; Dana, Hallock, Miller, Montague, Peterson, Pitkin, Trowbridge, Wilson and Wood, 1910; Davis, 1910; Eusapia Palladino, 1910; Hyslop, 1910; Jastrow, 1910; Krebs, 1910; and The Wonders of Palladino, 1910). In November-December of 1910 she had a series of sittings with an S.P.R. committee at Naples (Feilding, Marriott, Perovsky-Petrovo-Solovovo, Perovsky-Solovovo, Johnson and Baggally, 1911). Although no mention of Eusapia's mediumistic activities after 1910 is made in reviews of her mediumship published in English (Alvarado, 1982), it should not be assumed that she was not holding séances after that date. In 1911 Everard Feilding had a negative seance with Eusapia at Naples, as this investigator wrote in a letter to Hereward Carrington (1957, pp. 15-16). There are other reports of séances held in 1912 (Ponte, 1914), 1914 (Pegard d'Auriac, 1975), 1915 (Lucci, 1915, 1916), and 1918 (Vecchio, 1918). The purpose of this paper is to present in detail the above mentioned 1912 seance reported by Francisco Ponte (1914) in Spanish at a conference on psychic phenomena held in San Juan, Puerto Rico, in February of 1914.

Francisco Ponte was well known in Puerto Rico during the early 1900s. He was mentioned in a book of prominent Puerto Rican men in 1910 (Jackson and Son, 1910). From this publication we learn that he established himself as a successful dentist in 1900, and that he was active in politics around 1905-1908, a member of the San Juan City Council and temporary mayor (when he was 29 years old) in the absence of the elected mayor on one occasion. He later became involved in the activities of the Federación de Espiritistas de Puerto Rico (Federation of Puerto Rican Spiritists)<sup>8</sup> and became its president for 1913 (Rodríguez Escudero, 1978; Yañez Vda. de Otero, 1963).

In 1912 Ponte travelled to Europe, where he had a seance with Eusapia in Naples.<sup>9</sup> As seen in Ponte's later writings (Ponte, 1923, 1952), he considered his experiences in Italy as an opportunity to learn how to

study phenomena produced by mediums like Eusapia, since his only previous experiences had been with mental mediumship. This should be considered by those interested in evaluating Ponte's qualifications as an investigator of physical phenomena.

Ponte reported that he arrived one evening at Eusapia's home in Naples with a letter of introduction from the Italian psychical researcher Angelo Marzorati. But since Eusapia could not read and Ponte did not speak Italian, Ponte had to look for an interpreter. Having found one, he greeted Eusapia in the name of the Federación de Espiritistas de Puerto Rico and told her he wanted to see her "work" (i.e., produce phenomena). The medium told him she was rarely holding séances at the moment because of her age and health problems,<sup>11</sup> but that she would do her best to produce phenomena the next day. Not wanting to give Eusapia time for any preparation, Ponte told her that he needed the séance to be held that same night because he had to leave for Rome the next morning. Eusapia agreed, but she requested the assistance of Dr. Fiocca-Noví<sup>12</sup> for the séance. After Fiocca-Noví's arrival, as Ponte (1914) wrote:

"I began to search all of the medium's modest house, without even forgetting her wardrobe and we took out everything that was there, with the exception of the four chairs . . . , a quadrilateral pinewood table, and a mandolin that I placed in a corner of the little room. We improvised a dark cabinet in a corner of the room and placed a black curtain from one side to the other. We placed the table at two feet of distance from the curtain and seated the medium between it and an end of the table; at the other [end was] Doctor Fiosca [sic] Noví and at the sides the interpreter and I, holding with my right hand Eusapia's left and with my left, the doctor's right. I ordered the interpreter to do the same on the other side; both of us had absolute control of the medium and the Doctor, and we also had their feet under ours, so we could immediately notice any movement." (p. 33, my translation)

Ponte reported phenomena both with light and without light. When used, the light came from a small lamp and was of enough intensity so as to make it possible to distinguish the objects and persons in the room. The following is Ponte's report of the alleged paranormal manifestations that he observed in light:

"First - I felt strong blows from the table at the contact of our hands and those of the medium. Second - Repeated blows under the table, produced as if by a closed fist of a strong hand. As soon as the blows stopped, we were touched on the knees and the opened hand slipped down to the ankles. Third - Elevation of the table without contact to a height of 60 cm. (This

phenomenon was repeated five times at my request) Fourth - A well formed arm gave strong blows over the table with a hammer. I asked in Spanish if I could touch the hammer and I was immediately touched with it on the forehead, noticing that it was completely cold. Thinking that I could be under the effects of an illusion, I mentally said, so as not to inform the interpreter: 'You can touch with the hammer the gentleman at my left in the same way that I was touched;' immediately Doctor Novi said: 'I was touched on the forehead with the hammer,' showing that my mental petition was fulfilled. Fifth - The mandolin that I had placed on the opposite side of the hall played a piece of music, without my being able to see any person playing it. Then I asked: 'Would it be inconvenient if I bring the mandolin over the table to see and hear its sound from a closer position' [?] The response was that, to my surprise, a materialized arm moved the mandolin, suspending it over our heads and putting it later over the table [where it] continued playing." (Ponte, 1914, pp. 33-34, my translation)<sup>13</sup>

The record of the phenomena that happened in darkness reads as follows:

"The curtain was moved vigorously by a current of air perceived by all of us. The curtain was moved again and it wrapped around my head and I was dragged with the chair two feet towards the cabinet, from where a hand came out that gave me a few blows over my shoulder. I asked if they could touch the interpreter and immediately he was shaken so strongly that he was taken out of the chair, this affected him in such a way that he said he wished to leave. We calmed him with great efforts and implored the spirits not to make any further manifestations with him. Second - A hand was placed over my head, and caressed my face: it was like a hand covered by a very fine glove, or as if it were made of rose leaves. Third - Another hand unbuttoned my overcoat and drew out of the left pocket of the waistcoat a card that was bent in two parts and then replaced in the pocket. Fourth - A bust of a woman appeared with well materialized arms, but this was not the case with the head, so I could not distinguish her features. This bust stretched me between its arms, it kissed and talked to me, but I could not understand clearly the phrases pronounced. It seemed that it talked to me through a very distant telephone. I heard its voice, but it was almost imperceptible." (Ponte, 1914, p. 34, my translation)

### III

The phenomena reported by Ponte have numerous similar precedents in the investigations mentioned in the first part of this paper.

Although interesting, this report may be criticized for lack of details such as the dimensions and other characteristics of the séance room and, more important, a description of the control of the medium during and after the phenomena. After all, the séance was held in Eusapia's home (with the possibility of confederates or other deceptions) and the controllers at both sides of the medium were inexperienced in the varieties of mediumistic fraud, such as escaping control by substitution of hands in which Eusapia was quite proficient. However, it must be kept in mind that Ponte's account was not presented as a scientific report, but as an informal report of personal experiences presented at a conference of psychical phenomena for the general public.

In my opinion Ponte's report is important not as evidence for paranormal phenomena, but mainly for its historical value, since it provides us with additional information on Eusapia Palladino's last years. It is to be hoped that further research (probably in Italy) on Eusapia's last years may reveal new information about her performances and personal life that will no doubt be of great interest to students of the life and mediumistic career of this fascinating woman.

### NOTES

<sup>1</sup> I wish to thank Patric Giesler, Frances González-Scarano and Ian Stevenson for helpful suggestions for the improvement of this paper. I am also grateful to Eric J. Dingwall for general encouragement and for a reference to a séance with Eusapia published by Fiocca-Novati (1910), and to Emilio Servadio for correcting the spelling of some Italian names. Ugo Dettore and J. Fraser Nicol read the manuscript and gave encouraging comments.

<sup>2</sup> The spelling of Palladino has been somewhat controversial. As can be seen in the list of references at the end of this paper, some researchers write it with one "l", while others write it with two. The second alternative will be preferred in this paper for the following reasons: (1) Palladino is spelled with two "ls" in Eusapia's birth certificate (Nota, 1918); (2) Eusapia was illiterate, but she could write her name (Morselli, 1908, vol. 1, p. 124). Courtier (1908, p. 415) affirms she wrote her name with two "ls", and Carrington (1909, facing title page) presents a facsimile of her signature where two "ls" are clearly seen.

<sup>3</sup> For information on Eusapia's personal life see: Alippi (1962), Carreras (1918), Carrington (1909), and Miranda (1918). Eusapia's birth certificate has been published in Luce e Ombra (Nota, 1918).

<sup>4</sup> Other important reports will be mentioned in the next paragraphs of the introduction. The following publications, among others, may also be of interest: Blech (1897), Dariex (1896), Sabatier, Rochas, Gramont, Maxwell, and Dariex (1896), Samona (1903), Senigaglia (1910b), and Venzano (1907). For extensive bibliographical references up to 1907, see Morselli (1908, Vol. 1, pp. 134-170; Vol. 2, pp. xvii-xviii).

<sup>5</sup> For discussions of different aspects and perspectives of Eusapia's fraudulent activities see: Carrington (1910), Hodgson (1895), Hyslop (1911), Kellog (1910), Krebs (1910), and Ochrowicz (1896).

<sup>6</sup> Instrumental studies of paranormal physical phenomena were done before Eusapia's mediumship became prominent (e.g., Crookes, 1874, 1889; Hare, 1855), but her case provided the opportunity to extend previous efforts to a higher level of sophistication. Part of this may be explained by the rapid development of scientific instrumentation during Eusapia's time and by the more active interest shown by members of the scientific community in her case than in that of previous mediums.

<sup>7</sup> Vecchio's (1918) report is dated at the end of the paper as having been written in June of 1916. This seems to be a mistake. The date of the three séances mentioned is not given, but it is said that they were held on April 6, 14, and 22 (pp. 140-141), and that the last of them took place twenty days before her death (p. 145). Eusapia died on May 16, 1918 (Dingwall, 1950; Nicol, 1956), more than twenty days after the last of Vecchio's séances, but his reference to Eusapia's death in such a short interval of time suggests that the paper was written in 1918 and refers to séances in that year.

<sup>8</sup> The Federation was founded and held its first meeting in 1903 in Mayagüez, Puerto Rico (Memoria, 1903; Ramírez, 1913).

<sup>9</sup> Ponte's report is not clear regarding two points. First, it does not specify whether there were two separate séances or only one for the two conditions to be discussed later. Ponte (1923, 1952) wrote elsewhere that he had several séances with Eusapia. However, the point remains unclear. Second, it does not mention the date of the séance, although we can get a fairly good approximation by the following considerations. Ponte (1923, 1952) wrote that he visited Europe in 1912 and that while in Italy he had séances first with Lucia Sordi <sup>10</sup> in Rome and afterwards with Eusapia in Naples. Since Ponte had a séance with Sordi on September 5, 1912 (Bruers, 1912) and considering also that he reported being in Sweden on September 10 of the same year (Ponte, 1914, p. 15), it may be assumed that his visit to Eusapia took place sometime between those dates or not long before the first of them.

<sup>10</sup> Lucia Sordi was a controversial Italian medium mainly of the physical type about whom several papers were published in the second decade of the 1900s. Fodor (1933, pp. 352-353) presents some information about her. For more details, see: Le Nouveau Médium, (1911), Schrenck-Notzing (1911), Senigaglia (1910a), Tanfani (1910), and Tummo (1912).

<sup>11</sup> Eusapia was 58 years old when Ponte visited her. Regarding additional information about her health around this period, it is interesting to note a letter Everard Feilding wrote to Hereward Carrington in May 13, 1911 saying that Eusapia was not well and that her diabetes had worsened (Carrington, 1957, p. 16). In May of 1913 Eusapia was reported to be suffering from a serious "intestinal intoxication" (Pour la Venue, 1913). She died of nephritis (La Morte, 1918).

<sup>12</sup> Dr. Guido Fiocca-Novati was an Italian physician with several publications to his credit in the psychical research literature of the early 1900s (Fiocca-Novati, 1905, 1910, 1911, 1912a, 1912b, 1915). His 1910 paper reports a séance with Eusapia. It should be noticed that Ponte (1914, 1952) makes the consistent mistake of calling him "Fiosca" instead of Fiocca. Several other European names in Ponte's (1914) report are similarly distorted.

<sup>13</sup> Ponte continually changes from present to past tense in his verbs in this and in the following quotation. For consistency, I have used the past tense throughout my translations on his report.

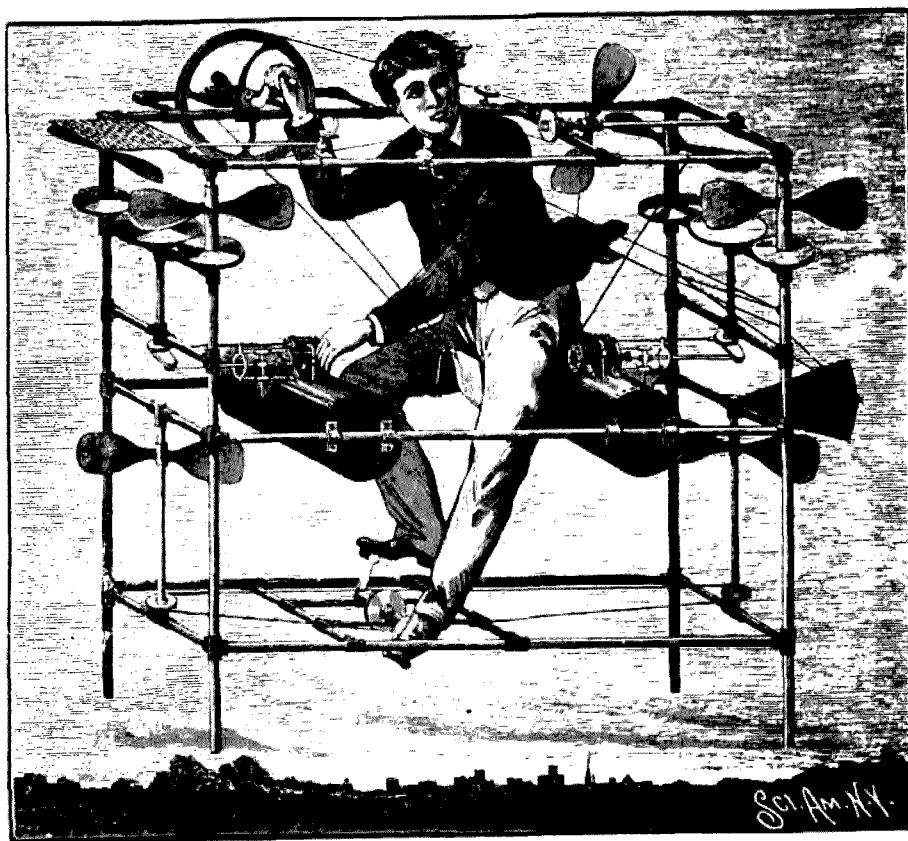
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## REFLECTIONS ON "PROJECT ALPHA": SCIENTIFIC EXPERIMENT OR CONJUROR'S ILLUSION?

Marcello Truzzi

### Prologue

A preliminary version of the following paper was sent to about 20 persons, including all the principal actors involved, to obtain their corrections and suggestions. Most responded, and this resulted in numerous changes. I am particularly grateful for the helpful critical commentaries supplied me by Drs. Peter Phillips, Michael Thalbourne, Berthold Schwarz, Walter Uphoff, Ron Westrum, Ray Hyman, and Stanley Krippner, and by Piet Hein Hoebens, Dennis Stillings and George Hansen. Though it is unlikely that my end product will be viewed as having satisfied all their criticisms, and only I should be held responsible for the interpretations put forward in this paper, the feedback they gave me on my preliminary draft resulted in some important changes and, I hope, an improved product. Unfortunately, James Randi replied to my invitation to correct any factual errors in the first draft with an angry and vituperative letter. In it, he stated that he would not respond to the questions raised by my article, for to do so would necessitate that he reveal information that would bring further embarrassment to the parapsychologists, and he wished to spare them that. If this is in fact the case, I can only urge him to exonerate himself at such expense to the psi researchers. I hope that upon further reflection, and upon reading this revision of the earlier draft sent to him, Randi might agree that reasoned dialogue is a more appropriate and productive approach to sorting out the many complex issues surrounding Project Alpha. I hope that he will publish a response to my analysis somewhere, and he remains welcome to space in *Zetetic Scholar*.

I fear that there are some who might read this essay and mistakenly conclude that I have sought to attack Randi on a personal level because several of the questions I raise concern his motives. Thus, parts of my analysis might be misconstrued as constituting an improper *ad hominem* attack. That is not my intention, and I believe that any questions I raise about Randi's motives are clearly linked to appropriate and relevant questions of evidence and argument. Despite our differences, and Randi's initial hostile reaction to my paper, I continue to believe that Randi has made and can continue to make important positive critical contributions to psi research.

Those who feel I have been too harsh with Randi will also probably conclude that I have been too kind to those foolish enough to be taken in by Project Alpha. Though I have tried to act as an honest broker between viewpoints, and I have aspired to be objective (recognizing that we seldom can be completely so), I have never claimed to be a neutral broker. The ground rules of science are conservative, and in so far as these place the burden of proof on the claimants and require stronger evidence the more extraordinary the claim, they are not neutral. But, we also need to remember, evidence always varies by degree, and inadequate evidence requires a tolerant reply which requests better evidence, not a dogmatic denial that behaves as though inadequate evidence were no evidence. I very much agree with Mario Bunge, when he stated that "dissent is of the essence of the scientific process, and the occasional pressure to suppress it in the name of the orthodoxy of the day is



even more injurious to science than all the forms of pseudoscience put together" (Bunge, 1980: 46). For some, Alpha has been an attempt to substitute ridicule for argument and evidence. By substituting horselaughs for syllogisms, we act to suppress dissent. It is for this reason that I am often more sharply critical of so-called "skeptics," with whose "orthodox" conclusions I may in fact largely agree, than I am towards the "maverick" scientists towards whom I have been accused of showing too much tolerance. We can afford to be tolerant towards honest players with maverick ideas if we believe the game of science is a self-correcting system where fair play will lead to correct judgement; but we can not afford tolerance towards those who play unfairly, especially those players for the orthodox side who start with all the advantages and thus threaten to close the game prematurely. My paper is not intended to end the game; it merely begins a new inning. This essay raises and unpacks what I believe are the proper questions; it is not offered as a set of final answers. Those who contest my observations, interpretations, and/or evaluations, especially those who were involved with Alpha, are invited to participate in the continuing dialogue in Zetetic Scholar.

Because the reflections which follow are complex, let me outline my analysis. My central concern is to examine Project Alpha on the very terms that Randi has asked us to take it: as a serious sociological experiment. Because the behavioral sciences have long been concerned with ethical issues surrounding research on human subjects, and because Randi's critics have raised questions about the ethics involved in Alpha, my analysis gives some consideration to such matters. But this essay is primarily a methodological analysis which assesses Alpha as a scientific experiment, not as a rhetorical drama. My essay considers four central questions: (1) Was Project Alpha a competently done scientific experiment? (2) Was Project Alpha ethically carried out in terms of the general values found in scientific practice, especially those which critics of psi research have endorsed in the past? (3) Was Project Alpha objectively and/or adequately reported? And (4) what is the significance of Alpha for our understanding of the process by which the psi debate is being evaluated within science? My analysis begins with a brief description of Alpha and the reactions to it. I go on to consider the context of Alpha, first in terms of historical precedents and then in terms of the challenge to which Randi was responding. I look next at the character of Alpha as a scientific "experiment." I then consider the actual impact of Alpha on the work at the Mac Lab and done by other psi researchers caught in Alpha's net. This is followed by consideration of several related episodes which have some implications for our assessment of Alpha: Randi's claim of a second successful experiment in Project Beta, his getting counter-hoaxed in a manner that revealed some problems with his approach, and two minor instances that raise further questions about Randi's methods. I then give my general conclusions about Project Alpha. Finally, in an epilogue, I seek to set the record straight about my own involvement with Alpha while it was in progress.

\* \* \* \* \*

#### The Revelation of Project Alpha

When Washington University's McDonnell Laboratory for Psychical Research made it known in 1979 that it was looking for psychics to be tested, young Steve Shaw (18) and Michael Edwards (17) applied and soon became star subjects. During several visits over the next two years they impressed the lab's director, Prof. Peter R. Phillips, and its experimenters with examples of

apparent psychokinesis (PK) and ESP. At a press conference on January 28, 1983, the conjuror James ("The Amazing") Randi (1983a) revealed (that the two wonder workers were part of his Project Alpha" (soon dubbed the "Shazam Scam" by some of the press) and that the boys were skilled conjurors who had conspired with Randi to fool the researchers. Project Alpha, Randi asserted, demonstrated the inadequate controls used by parapsychologists against fraud as they pursue what Randi characterized as their "claptrap science." Randi claimed the boys had bamboozled the parapsychological communities in both the United States and England. Further, he said the boys had been instructed to reveal the truth if asked if they were faking, but they simply were never asked. As evidence of success, Randi cited an "article" by Phillips in Research in Parapsychology, a supplement to the Journal of American Psychosomatic Dentistry and Medicine by Dr. Berthold E. Schwarz, and several stories about the boys' powers in The National Enquirer. Randi announced the full story would appear in the March issue of Discover magazine (Anonymous, 1983a) and also as part of Randi's TV special, "Magic or Mystery?" to air on February 8th. At his Discover press conference, Randi also warned parapsychologists that a "Project Beta" was "already underway" which Randi said he hoped would fail because of the lesson learned from his Project Alpha.

The reaction to Randi's announcement was a mixed one. The most extreme positive reaction was that voiced in Discover (Anonymous, 1983a) where all parapsychologists were ridiculed, where it was suggested that the American Association for the Advancement of Science should seriously consider expelling the Parapsychological Association from affiliation, and it was concluded that "it seems clear that most of their [the parapsychologists'] experiments are poorly controlled, that their published reports are naive, if not deceitful, and that neither qualifies as science." [When I discussed this essay with Randi, he said it went further in its generalizations than he would endorse, and I was told by mutual friends that Randi was unhappy with its extremism; but it is noteworthy that Randi never publicly disclaimed Discover's coverage in the form of a published letter to its editor or in his later reports (Randi, 1983d and e). Thus, it was natural that most readers would assume that the Discover column reflected Randi's own views properly, especially since he did "invite" those at his press conference to read the "detailed account" in Discover, issued his press release through Discover, and publicly thanked Discover for "having maintained their silence" during the ongoing experiment, thus indicating that the magazine was, to some degree, a collaborator with Randi in his project.]

Less extreme, but certainly delighted, was the response of The Skeptical Inquirer. Project Alpha was the featured cover story in its Summer 1983 issue. Martin Gardner (1983) wrote of the Alpha hoax as a "landmark in the history of PK research." Randi published his account of the events in its Summer and Fall issues, and he later presented a further account (in two sessions) at the October 1983 conference on "Science, Skepticism and the Paranormal," sponsored by the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP), publisher of The Skeptical Inquirer. Though Randi's project apparently was done on his own and conducted independently of the CSICOP, his prominent role in it, the apparent endorsement of his project by its journal, the featured role Alpha was given at CSICOP's conference, and knowledge of Alpha by some Fellows of CSICOP prior to Randi's public revelation, have all caused many to perceive Alpha as a CSICOP project. To date, no statement disassociating CSICOP from Alpha has been made to counter these impressions, but I would stress that there is no formal connection between Alpha and the

At the other extreme were those "parapsychologists" (e.g., Professor Walter H. Uphoff) and their friends who cried "foul" and even continued to insist that some real PK phenomena had occurred in conjunction with (if not directly caused by) Randi's young accomplices, despite the boys' denials and Randi's claim of a thoroughgoing debunking (e.g., Schwarz, 1983b). In addition, Randi was exaggerating --as when he claimed to have "bamboozled the parapsychological communities in both the United States and England" but only offered dubious evidence of that for the U.S. and none for England. He also ignored the last experiments with the boys--reported on at the 1982 Parapsychological Association meetings--where controls seem to have been adequate, and where the boys say they used no trickery, and the results were not indicative of psi (Thalbourne and Shafer, 1983; Shafer, 1983; and Shafer, *et al.*, 1983; also, cf., Thalbourne, 1983).

More common was the middle ground taken by some (e.g., Auerbach, 1983; and Hölvelmann, in press), including myself, Peter Phillips, and some prominent members of the Parapsychological Association (e.g., Stanley Krippner), who felt that there were potentially constructive aspects to Alpha for parapsychology but that serious ethical issues were involved, also. If Randi had been a psychologist and not a conjuror, some pointed out, he very possibly might have been expelled from the American Psychological Association for what would surely be viewed by many as unethical interference in another scientist's research program (cf., Broad, 1983). The norms within the psychological community about such matters are far from clear, but had Randi been a psychologist and APA member, sanctions against him could have been sought and perhaps obtained. But Randi is not a professional scientist, and the norms defining proper behavior for him are even less clear. As with many matters, Alpha is not something to be viewed in simple black and white terms. It is a complex matter and by no means a novel one for psychical research and thus should be examined in its historical context for proper understanding.

#### Precedents and the Problem of Parsimony

Reading the extreme critics and defenders of Randi, one might get the impression that hoaxes like Randi's had never taken place in science before. In fact, of course, there have been many hoaxes within science (cf., MacDougall, 1958) and many of them contain strong parallels to Alpha (I do not suggest that there are perfectly comparable cases). For example, a well known case was that of the University of Wurzburg's Professor Johann B.A. Beringer that began in 1725 (cf., Jahn and Woolf, 1963). Beringer deeply believed that fossils were merely "capricious fabrications by God" probably put in the earth to test man's faith. To demonstrate his gullibility, some of his students (one of them in the employ of his rival) forged absurd clay tablets with all sorts of inscriptions in ancient languages. They even put the signature of God himself on the fossils. Beringer began to produce a very expensive book on the fossils, and the students told him the truth; but Beringer refused to believe them and went ahead with his book which was then met with laughter and ridicule. An official inquiry was held, and punishment was given to the hoaxers who had sought to make Beringer a laughingstock "because he was so arrogant." In this case, sabotage of a scientific research program was dealt with harshly.

Perhaps the strongest parallel case within early psychical research was the hoax pulled by Richard Hodgson, a leading psychical researcher and an editor of the *Journal of the Society for Psychical Research*, and S.J. Davey (cf., Hodgson, 1886-7 and 1892; and Davey, 1887 and 1888). Davey had found he could duplicate some of the effects of the seance room through trickery, and Hodgson set up a series of seances for psychical researchers (including some of the most famous of the period) at which Davey acted as medium and practiced his deceptions. The invited researchers all wrote up their observations describing what they believed to be real spirit activity that could not be produced by trickery. Later, Davey and Hodgson revealed that they had been deceiving everyone as an experiment to test the validity of reported observations. This experiment produced an uproar since the reports were highly embarrassing to those who made them. In many cases reports were made of things either not done by Davey or were reported to have occurred in ways that precluded the manner in which he actually did them. Many complained about the fraud, and some insisted that real phenomena had been produced at these seances despite Davey's disclosure of fraud. Though largely forgotten today, this early study by Hodgson and Davey had a great impact at the time, and many critical researchers considered it extremely important. Not only did it produce serious questions about human testimony, it acted as a de facto control group comparison which reduced the credibility of prior reports made by these same "witnesses" about other seances.

In such cases as the above, the fooled participants felt victimized and considered the fraud sabotage. On the other hand, the perpetrators of the hoaxes felt that what they were doing was for a higher good, to demonstrate incompetence by the researchers. A noble end was the justification for ignoble means. There seems little question that there have been past cases (e.g., the debunking of N-Rays) where such a deceptive approach was productive for science. The general issue of lying to human subjects in psychological research is a serious one within the social and psychological sciences, and this is a complex problem. The problem is made even more complex when we have one set of researchers conducting uninvited experiments on another set of researchers and lying to them as well. (Another important dimension to the problem, one Dr. Michael Thalbourne has called to my attention, concerns the issue of whether competence should be measured the same way when we are dealing with exploratory research that is clearly defined as informal and of pilot character, as much of the early Mac Lab work seems to have been.)

In dealing with such cases, any judgement of the ethics involved needs to include consideration of the intentions and motives (usually complex and perhaps always incompletely determinable) of those conducting the fraud. Was the fraud done in the hopes of "catching" the researchers doing incompetent work? How objective and disinterested is the party introducing the fraud? If the fraud had been detected by the researchers, would the results have been published and the researchers commended for their competence? Was the goal in using fraud to bring ridicule to the research and perhaps block future research, or was it done with the goal of promoting improved research? Has the introducer of the fraud clearly stated in advance what the possible results would mean? These are the sorts of questions that need to be asked before we can really judge the character of such "experiments." We might wish to distinguish some episodes as "hoaxes" (meaning they were meant merely as jokes) versus others which we label "frauds" (where more serious results were expected or intended). We should also remember that the key question of whether

fraud occurred must also ask how much occurred and whether there were any validated extraordinary effects that remain unaccounted for by the efforts of the tricksters.

Unfortunately, these questions are often unanswerable about past episodes. For example, it appears that Douglas Blackburn and George Albert Smith (according to Blackburn's 1908 confession) used trickery in 1882 and 1883 to fool many leading psychic researchers into believing that they had powers of telepathy. Although I find the evidence overwhelming that they used deceptions (cf., Hall, 1980), Smith's denial of Blackburn's confessions remains believed by some even today (e.g., Delin, 1972). In such cases, we need to weigh the conflicting evidence and arguments as best we can, but we are usually forced to rely on parsimony for our conclusions.

Parsimony, choosing the simplest adequate explanation, must ultimately act as our criterion for judgement in such cases. Unfortunately, the judgement as to what constitutes a more parsimonious conclusion usually involves subjective elements and a degree of social negotiation (cf., Collins, 1976). If history has shown us anything, it is that disclosing that fraud took place is unlikely to convince all those fooled. As with Beringer and those fooled by Davey, some people will simply not accept the revelation of fraud as an adequate explanation. This refusal by a strong advocate to admit that he was fooled (made a fool of?) seems common. An excellent case is that of David Jones who recently displayed his fraudulent perpetual motion machine to some "perpetual motion freaks" (Jones' term). They welcomed him into their brotherhood, and, as Jones described it: "I protested that my machine was a hoax, and that I was a self-confessed charlatan; even so, I was once accused of lying to protect my secret. I fear that many of the poor freaks must have returned to their workshops with renewed determination" (Jones, 1983).

An irony in such cases is that once the defrauders admit to their fraud, the door is open to distrust them entirely. Some proponents will demand that the defrauder prove that he committed the fraud claimed and will not simply accept his word for it. Thus, some scammed by Randi (and it should be emphasized that Professor Phillips is not among these) insist that he has not yet adequately demonstrated how each and every seemingly paranormal effect was produced by deception; and until he does so to their satisfaction, they simply will not accept his claim to successful fraud. On purely logical grounds, theirs is a tenable position in light of the many loose ends involved. But for most of us, the parsimonious conclusion (the one psychologically easier since it seems to make less new assumptions about the world) is that we should believe Randi and his associates when they merely tell us they produced all their effects through quite normal means. Given his lack of knowledge about conjuring and his spiritualist views, Sir Arthur Conan Doyle could logically argue that Harry Houdini had actually secretly used paranormal powers of dematerialization to escape from his bonds and that Houdini was lying when he claimed to do it through trickery (Conan Doyle, 1930), but most of us view such an explanation as ludicrous because it strongly violates what we see as the more parsimonious one that Houdini escaped quite normally, just as he claimed, even though we may not fully know his methods. Nonetheless, it must be acknowledged that without knowledge of his methods (in actual fact, Houdini's methods have long been well known among conjurors), acceptance of Houdini's word that he did mere tricks must be taken on trust; thus, Conan Doyle's position may seem foolish, but it was not irrational.

We need to recognize that those (e.g., Schwarz 1983b, who conducted research only on Shaw) who today think that Randi has not yet adequately explained the details of Shaw's and/or Edwards's tricks and who continue to believe that real PK took place and that Randi's debunking claims are inadequately supported are neither dunces nor irrational. Those of us who accept Randi's claims as thoroughly discrediting the reports of the boys' PK can not do so on purely rational grounds. We do so because we think our conclusion is the most reasonable under the circumstances, not because our logic and evidence are truly airtight. Until each and every instance of alleged PK by Shaw and Edwards is adequately accounted for as to how it was actually normally produced (not to the researchers fooled—they may never become convinced due to normal factors of human errors of memory and inference—but to the rest of us making a judgement), we may still reasonably take their word that they cheated; but we must recognize that such trust is an act of faith and not one based on purely objective or strictly rational grounds. (Many of the simulation methods are generally available; what some feel is needed is an explanation of the few events which Schwarz and Uphoff still maintain might have been genuine examples of psi.)

If Randi truly expects us to view Alpha as a "serious sociological experiment" (Randi, 1983a) that provides us with disproof, and not merely as a publicity prank intended to discredit claims of PK, he must at some point publish the details of the simulation methods used. Otherwise, how are independent, objective observers (especially those outside of the conjuring fraternity who know little about the limits of trickery) to go beyond a merely reasonable or plausible conclusion (agreeing with Randi) to one that is a truly scientific (experimentally established) judgement? None of this is to argue that Randi must reveal his methods. As a conjuror, he has the right to guard professional secrets; he might even be condemned by many fellow conjurors if he chose to reveal all. But it must be openly acknowledged that this is a basic limitation placed on such claims of a conjuror that keep them from equalling a proper scientific claim. Unless Randi is willing to bear the full burden of proof about his claim that all reports of the boys' PK effects can be explained by cheating, his can at best be only a quasi-scientific claim; and those of us who accept the claim should recognize that we do so based on what I believe is a reasonable bias rather than upon purely objective (i.e., exclusively scientific) grounds.

Lest I be accused of "mystery-mongering," or somehow being an apologist for those who insist that real paranormal events have occurred even where they can be or have been substantially duplicated by trickery, I must categorically state that such is not my purpose in the above discussion. I only wish to point out that the issue in such matters revolves around questions of parsimony rather than logic *per se*. Many critics apparently believe that duplication through trickery is sufficient to discredit a claim that an effect was produced paranormally. Such duplication by trickery certainly is sufficient to raise serious questions about a paranormal claim, but a question remains a question and not an answer. The burden of proof is upon the claimant in science, and those who make claims of the paranormal must bear that burden, not the critics. But when a critic rejects a claim not because the evidence for it is inadequate for the size of the burden it must bear (and parsimony then plays a role), but because he claims to have proven the evidence is the result of trickery, he is himself making a positive claim and that claim, too, needs proof rather than mere assertion. It is reasonable for us not to expect a conjuror to reveal his methods to us, and it is not necessary for him to do

so if he merely wishes to reduce the weight of the evidence for some paranormal event about which his duplication through trickery raises serious questions. But, I would contend, it is not mere nit-picking, nor is it apologetics, to remind us that much of our discourse in such matters rests on issues of parsimony and therefore must leave the door slightly ajar for what to many of us would appear to be highly implausible arguments. To close that door would be to block further inquiry, and that a true scientist must never do.

### The Background and the Challenge to Randi

It must be understood that Randi's Project Alpha was in large part his response to a challenge that had been frequently made to him by several parapsychologists. In the past, Randi—as other conjurors before him—usually presented evidence that some alleged psychic effect had not been produced paranormally by himself producing a similar effect through deception. Most critics of psi accepted such a demonstration as a replication of the alleged psychic effect and adopted the parsimonious conclusion that the psychic's effect was probably also a trick. Often, there was controversy over the degree to which the conjuror actually reproduced the same effect under similar conditions. Critics of psi, who usually adopt stringent criteria for replication when proponents claim favorable instances, sometimes accept very loose criteria for replication when the claimed reproduction is discrediting to psi. Thus, Randi's simulation of psychic metal bending on a television show might be accepted as discrediting similar effects reported to have been done under more controlled conditions. But even granting comparable conditions, proponents of psi point out that similar effects do not necessarily imply similar causes. The existence of wigs does not negate the existence of real hair. Randi has always acknowledged that his ability to reproduce psi effects through deception does not prove the effects he duplicates were identically produced; Randi clearly states that his argument that the original effect was probably also produced by fraud is based on parsimony (reasonableness) rather than pure logic. As Randi put it: "I have never claimed—nor could I, as logical person claim—that my duplication of 'psychic' feats shows that 'psychics' use similar trickery. What it does show is that it is more rational to suspect trickery than to adopt the preposterous alternative." (Randi, 1980, p.3). Note that here Randi says "suspect trickery" rather than "assume trickery." His formally stated position is not so severe as many assert. Nonetheless, Randi's less formal statements frequently sound as though he assumes and even asserts that trickery is the explanation for an alleged paranormal phenomenon; thus, Randi has contributed to the confusion about what he really has been saying. Most psi researchers probably would not take issue with Randi if he consistently stated that he merely suspected trickery as he does in the above formal statement by him. It is clear that Randi goes beyond mere suspicion, for he believes trickery is the most probable explanation, not merely a reasonable conjecture.

Because of this problem, numerous proponents had demanded that Randi come into their laboratories, under their conditions, to demonstrate whether or not he could truly replicate the reported test performances of allegedly real psychics. And when Randi refused that challenge, they viewed this as demonstrating that he was unable to reproduce the phenomena under truly similar conditions. The obvious problem with such a challenge, of course, is that the conjuror has no guarantee that the conditions would truly be similar. In the first place, the actual conditions under which the allegedly real psychic was

tested can not be truly duplicated. Those conditions were necessarily incompletely and perhaps inaccurately reported. (Ray Hyman has pointed out that even the "psychic" who originally performed a feat could not repeat his performance under exactly the same conditions, so a proper test would require that both the "psychic" and the conjuror attempt the same feat under conditions to be agreed upon and juried by a panel of proponents, critics and agnostics.) Probably more important, knowing they were dealing with a conjuror, it seems likely that the researchers would be particularly on guard and would not likely seek to be "helpful" as they might be to someone they thought a genuine psychic, one out to advance rather than discredit their work. The atmosphere would be totally different, and that would be very important for anyone seeking to misdirect the researchers.

Knowing this, Randi still tried to live up to the challenge as best he could. Thus, in 1975 he visited the offices of England's spiritualist newspaper *Psychic News*, got himself introduced as a real psychic named Zwinge (his original family name) and completely fooled them into thinking he was a major new psychic find (Randi, 1982, pp. 186-190). Since he was able to hide his real identity, Randi demonstrated that he could deceive some persons supposedly experienced in psychical matters. But, of course, such spiritualists should not be confused with serious laboratory scientists. How then could Randi accept the latter's challenge while guaranteeing the same conditions that they might give a so-called "psychic" like Ted Serios or Uri Geller? Randi's answer was Project Alpha.

In an independent analysis of the issues involved, sociologist Trevor J. Pinch (1979) argued that to demonstrate that fraud took place in the original experiment, "the results of the replication must first be published as a paranormal claim, just as parapsychologists themselves have done.... If those claiming fraud do not get their initial (apparently) paranormal results published, then the critic can say that fraud has not been unequivocally demonstrated because the paranormal interpretation of the results was unconvincing. It is as though a magician pulls a rabbit out of a hat without showing us first that the hat was empty. No replication of fraud which meets this condition has yet been reported—at least none which warrants scientific attention" [in a footnote, Pinch indicates he here is making reference to Randi's episode with *Psychic News*] (Pinch 1979, p. 336). Though not directly in response to this critique, Randi's Project Alpha began as an attempt to meet this very problem.

### The Nature of Alpha

Essentially, Randi introduced trickery into an ongoing research enterprise in order to expose publicly what Randi considered to be the absence of proper scientific procedures that should have controlled against or discovered cheating. It is important to emphasize the following point: The only thing one might label "fraud" found during Alpha was that put into the situation by Randi (and one can even argue about the use of that term here since Randi's gain from Alpha was not directly related to any financial loss to Alpha's victims, he did not intend that they suffer financially, and he did expect to eventually "debrief" them). Alpha was not done to lure the experimenters into fraud or to get them to act dishonestly. Thus, it was not a case of entrapment; it was merely one of sabotage. But this sabotage was conducted in a "good cause," to detect and reveal what Randi saw as research incompetence.

Therefore, such sabotage was viewed by Randi as excusable. (I must emphasize that I do not view malice as a necessary part of the definition of sabotage, as those who are offended by my use of this term have inferred; sabotage can be a defensive act as well as an offensive one, as when a spy sabotages an enemy effort to bomb his homeland.) And, of course, if the researchers' controls had been adequate to stop fraud, Alpha would have failed and (presuming that Randi's association with Alpha become known) Randi would have been the one discredited once his failure to fool them had been revealed. (In fact, we must remember, too, that Dr. Thalbourne of the Mac Lab argues that the formal experiments they conducted were fraud proof and that Alpha therefore actually failed, despite Randi's claim to the contrary.)

It seems to me that those who applaud Alpha can be usefully categorized as taking either one of two general views towards Randi's hoax. One is that we are dealing here with a magician, a non-scientist, performing a useful role in debunking what he believes to be pseudoscientific claims by paranormalists. As such, it is an independent action by a performer, partly taken in response to challenges made to him by antagonists. As such, it would be inappropriate for us to raise serious questions about scientific ethics and methodology. This seems to be the attitude many take towards Alpha. It was merely a prank with a useful side and a prank well done in that it brought ridicule to those who demonstrated their incompetence. (One version of this viewpoint take the position that Randi's argument was mainly rhetorical and not purely scientific; it is to remind us that when a scientist of good credentials attests to paranormal effects going on in his laboratory, we should take that with a large grain of salt.) Viewed thus, it would fall into the same category as the famous instance of an art show jury that was fooled into bestowing its first prize upon a "work of art" that was actually a painting by an ape which had been entered by some art students. It mocks the pretentious, and many get a good laugh out of it, especially those who think the victims were silly in the first place. If this were all Randi had intended, it would be difficult to take much issue with Alpha.

We must reject this view of Alpha as just a clever prank, however, since Randi emphatically tells us he did not mean it as a mere joke. He even pointed out in his press announcement (Randi, 1983a) that: "It must be stressed that Project Alpha was designed as a legitimate, serious sociological experiment. It was not intended to embarrass or belittle any persons." It also seems clear that many in the scientific community (at least at Discover and at the CSICOP) seem to have accepted this lofty description by Randi of what Alpha is supposed to be. In light of this, Alpha must be evaluated as a serious experiment before we can judge whether it was successful or a failure.

As I noted earlier, there have been past cases where fraud has been used within science to catch incompetents. Perhaps the most publicized recent case of this sort was that in which researchers feigned mental illness in order to get into a mental hospital so that the diagnostic practices of the psychiatrists could be tested (Rosenhan, 1973). These "pseudo-patients" found that once they had been labelled psychotic, they could not establish that they were sane, even though they were quite normal. Though there has been much controversy over the ethics involved in that study, many scientists believe that the deceptive means used here were justified by the study's important ends. There are many other parallels. For example, it is not uncommon for those interested in security (as in industry or in intelligence work) to employ third parties to ascertain if they can break through security precautions to test the sys-

tems. It could easily be argued that parapsychologists have some responsibility to similarly test their controls against fraud by actually promoting tests of their precautionary measures by potentially helpful cheats. Parapsychologists must produce adequate controls against error and fraud if they are to convince the generally skeptical scientific elites of the value of their experiments. (I speak here of formal experiments. We must remember that a substantial case--but, in my view, not a clear cut and therefore convincing one--has been made that the early work at the Mac Lab was in fact not of a formal character.) Thus,, those who value truth more than they dislike ridicule should--at least to some degree--be grateful for any exposure of inadequate controls by those in their midst. We need to assess Alpha in light of such considerations.

#### The Mac Lab and the Rumors

At the 1981 meetings of the Parapsychological Association, Prof. Phillips gave a two part presentation. During the first part, Phillips showed some tapes he had obtained from Randi which demonstrated how metal could be bent through trickery. These were tapes Randi put together at Phillips' request (it needs to be noted that Phillips actively sought Randi's advice prior to this convention, before it was forced upon him as one might infer from Randi's accounts) and included footage from television appearances, including some of those by Uri Geller. Following this first part, Phillips showed us tapes he had of some experiments on PK done at the Mac Lab with Shaw and Edwards. The reaction to Phillips' presentation from his fellow parapsychologists was clearly hostile. Many asked him how his tapes really differed from those from Randi. They criticized him for his lack of controls in the experiments. Charles Honorton even stood up and said that this kind of work was setting parapsychology back many years. Randi sat next to me during all this, and he acknowledged to me that though Phillips had been fooled by the boys, the bulk of those parapsychologists present clearly had not been impressed. Randi even told me that he was proud of the parapsychologists for the way they showed their skepticism. Since Randi told many of us at the PA meeting that he was at the convention to write up his impressions for *The Skeptical Inquirer*, I told him I expected to see him write up the matter for that journal saying what he had said to me. As it turned out, Randi never published any column about this convention at all. I have recently been told by correspondents at the Mac Lab that Randi's failure to publish the expected column may have been the result of his prior agreement with Peter Phillips not to write anything about the presentation since it was to be an informal session to which reporters were not being invited.

A strange side event at this convention was that all sorts of rumors started up about what might be going on. The most frequent one I heard was that since the Mac Lab films were so terrible, Randi and Phillips must be collaborating on some sort of experiment at the convention to see if the parapsychologists would fall for such stuff. Given that Randi's films showed methods so similar to those that could have been used by the boys in Phillips' films, it was quite understandable that some of these conjectures started going around.

Finally, it should be mentioned that Randi's complaints about the Mac Lab work center around these early efforts presented at the 1981 PA conference.

Professor Phillips correctly points out that his lab's best work was done after this presentation, work during which they took the critical advice of Randi and fellow parapsychologists and during which no fraud (or very significant psi results) took place. Phillips argues that characterizations of him and his colleagues as "incompetent" might more accurately have described them as "inexperienced but capable of learning as they went along."

#### The Reactions to Alpha

Project Alpha's success was soon questioned. Washington University's spokesman quickly pointed out that the Mac Lab had never issued any scientific endorsement of the boys' psychic abilities, that nothing had been published about the boys in any peer-refereed journal, and he also cited a September 1981 statement from the Mac Lab (Phillips, 1981a and b) that nothing the boys had accomplished could not also be done through normal means. Questions also were raised about the claim that the boys had never been confronted with inquiries as to whether they used trickery. (I always found Randi's charge that the boys were not even asked if they were cheating a bit silly. Even if I suspected Uri Geller of fraud, I would not expect him to admit such to me just because I bothered to ask him. Did Randi bother to ask Geller if he cheated before Randi wrote his book exposing Geller? It is, after all, not the job of interrogation but that of proper experimental controls to eliminate cheating. Besides, just because the boys were instructed by Randi to admit fraud if asked, (a) what reason would the experimenters have had to expect that, and (b) why is Randi so certain that the boys obeyed his directive?) Other parapsychologists recalled Randi's presence at the 1981 meeting of the Parapsychological Association when Phillips' research with the boys was sharply criticized, both from the floor and afterwards during informal conversations. They expressed surprise at Randi's apparent insistence (inferred from his media statements as in Anonymous, 1983a) that Phillips' early work was typical or representative of the best in the field.

It soon became apparent that Randi's hoped for big fish--the endorsement of the boys' PK by a major psi researcher who had previously claimed experimental validation of other wonder workers--had gotten away. Randi was only able to show off a couple of minnows he had caught. Perhaps this was in part because Randi had prematurely closed down his "experiment" before a big fish could bite and Randi decided to have a report on Alpha filmed in time to make the deadline for an upcoming television special on which he was to be featured. On the other hand, the Mac Lab had finished experimenting with Shaw and Edwards some months earlier, and reports indicated that Professor John Hasted (a prime potential target for Alpha) was acting most cautiously because rumors that the boys were fakes had begun to spread.

As I have already emphasized, we must remember that the only "fraud" Randi incontrovertibly exposed in parapsychology was that which he placed there himself. Martin Gardner (1983-4b) has referred to Alpha as a case of "entrapment" which Gardner cites as being "the act of catching in a trap or luring someone into a compromising statement or act." In the usual, especially the legal sense, we speak of persons being entrapped when they are lured into some wrongdoing or crime. Though Alpha did reveal much credulity and even gullibility among a few parapsychologists, the facts remain that (1) Randi has not reported that he found any fraud committed by psi experimenters; (2) some Mac Lab researchers may have personally believed in the PK abilities of the

boys, but they never claimed any scientific validation of their abilities and, in fact, issued a major disclaimer to that effect (Phillips, 1981a and b; Bannister, 1983); and (3) Randi saw the negative reactions of the parapsychologists at the 1981 PA convention to the Mac Lab work, so he knew full well that Phillips' experiments were neither typical nor representative of the best work being done by parapsychologists. (To his credit, Randi did indeed expose what many of us would characterize as the gullibility of some parapsychologists, but for this he received praise rather than condemnation from several leading parapsychologists for the useful reminder to all of the need for guarding against tricksters.) It is also important to note that the McDonnell Lab reportedly (Lipowicz, 1983: 18; and Futterman, 1983: 13) spent \$10,000 to host and test Randi's young frauds. A legal action could have been brought against Randi for those costs, but the Mac Lab chose to avoid further expense, time and publicity. To say the least, Project Alpha raises serious questions about the ethics of such an "entrapment" operation done in the name of science.

#### Randi's Minnows

The chief victim of Project Alpha was Dr. Berthold Schwarz, a psychiatrist who was thoroughly convinced of the validity of Steve Shaw's psychic powers. I have never met Dr. Schwarz, but from all reports, he is an unusually warm and trusting person. I suspect that his great sympathy for people would make him an excellent therapist. Unfortunately, such traits can make one particularly ripe for a charlatan ready to take advantage of one's trust. Dr. Schwarz was an early member of the Parapsychological Association and has published much about psychical research and about UFOs. Nonetheless, he is not an experimentalist and his work is not typical of that done by experimental parapsychologists in the PA.

On the one hand, Dr. Schwarz was thoroughly fooled by Steve Shaw, and he even wrote a monograph (withdrawn prior to official publication) claiming to validate Shaw's abilities. Clearly, his poor observation was demonstrated by Shaw's subsequent debunking revelation. But the fact is that Dr. Schwarz's claims were never endorsed by his peers in the PA, and his writings about Shaw never were published in a peer-refereed journal. More important, Dr. Schwarz had not previously validated any other youthful metal bender who might now be discredited in light of his mistakes with Shaw. So, this was hardly a major catch for Randi. Note should also be made of the substantial expenses Dr. Schwarz must have incurred in bringing Shaw in for testing, etc. Randi's scam must have cost Schwarz more than pride, so we can hardly be surprised that Schwarz is not grateful to Randi for his "lesson" from Alpha.

Similarly, even if we accept (as I do) that Randi succeeded in discrediting some claims of independent psi researchers like Professor Walter Uphoff --who are not members of the PA, do not represent its standards, and whom Randi today would no longer even call "parapsychologists"-- that hardly constitutes the sort of massive indictment of parapsychology that *Discover's* column would suggest. Nor is it the "landmark in psychical research" that *The Skeptical Inquirer* labelled it. In short, Randi's biggest trick with Alpha may have been the illusion whereby he made such a mole-hill appear to be a mountain.

Finally, it should at least be mentioned that a great many questions have

been posed by the rejoinder to Randi from Dr. Schwarz (1983a and b). For example, Dr. Schwarz first became involved with Shaw because Schwarz hoped that Shaw might be able to use PK to help his seriously ill daughter for whom no orthodox medical cure is available. According to Schwarz, Shaw encouraged both Schwarz and his daughter in Schwarz's quest for a paranormal cure for her otherwise hopeless condition. When I later asked Steve Shaw about these charges (we spoke at the 1983 CSICOP conference where he was a featured speaker), he flatly denied this description of the events that occurred. Knowing Randi's past condemnations of the claims of psychic healers, I wrote to Randi when I first heard about Schwarz's complaint, and I asked Randi to confirm or deny Schwarz's allegations. Randi wrote me back saying he refused to reply to such insulting charges. Some of these issues seem to be matters of public record, as when Shaw was taken by Schwarz to the National Institute for Rehabilitation Engineering where Shaw purportedly explicitly spoke about the potential of telekinetic forces for therapy. According to Dr. Schwarz, Shaw's presentation on that occasion is available on tape and corroborates Schwarz's version of this matter. Whatever the facts may be on this issue, there is no reason for us to assume that Randi necessarily always knew exactly what Shaw said to Schwarz. Shaw may not have adhered to Randi's instructions at all times. Since there appears to be objective documentation that might at least partly resolve a dispute that so far has hinged on differences in recollections (and memory is notoriously susceptible to error), we may yet learn what really happened. I hope that Randi will eventually get around to giving us his own version of what may have happened.

#### Project Beta and Beyond

Though Project Alpha limited its trap to a small set of psi researcher victims, Randi's announcement at his Discover press conference of a Project Beta already in progress was a warning issued to all. Many researchers reportedly reacted to this announcement with mild paranoia. People within psi research abruptly became suspicious of one another, and all sorts of rumors started flying about where Randi might strike next. I was told that some people had even decided to postpone research projects because they simply did not want to get entangled with Randi. In one instance, as the story came to me, a benefactor from the United States gave a British university around \$5000 most of which was returned because a key researcher there mistakenly feared it might be part of Randi's plot.

Finally, in August of 1983, Randi revealed what he claimed was both the character and the "success of Project Beta." In his published statement on Beta, Randi wrote that "Beta had all along consisted of waiting to see if (a) the parapsychologists would recognize the need for competent outside help from the conjuring profession, and (b) whether they would actually fulfill an announced intention of seeking that assistance" (Randi, 1983-4a, p. 103). Since the Parapsychological Association had passed a resolution at its recent convention to invite conjurors (via the major magical organizations) to assist in controlling against deception in experiments, and since Prof. John Beloff had asked Randi for his help in setting up controls in a PK experiment, Randi then "happily announced" that Beta was "now terminated and with great success." Randi's announcement included kind words for many, indicated that he would no longer refer to psi researchers who were not members of the Parapsychological Association as "parapsychologists" (an important matter since Randi had in the past always publicly equated serious researchers, most of whom are

in the PA, with the often silly psi seekers that pollute the field), and said he looked "forward to a growing relationship with those who have recognized a genuine move toward a more complete understanding between both camps." Unfortunately, all this sweetness and light ended with a short paragraph in which Randi let everyone know that he had also started an as yet uncompleted "Project Gamma" which might eventually be revealed. About this new time bomb planted among the psi researchers, Randi has thus far revealed only that it involves a group of scientists and a study begun two years before. So much for Randi's "growing relationship" and a new spirit of "understanding."

Randi pointed out that some "jittery parapsychologists" had "jumped to the conclusion that Beta must be of the same nature as Alpha, quavered that I had inhibited further research by this second project. They were quite wrong" (Randi, 1983-4a, p. 102). But were they? The evidence strongly suggests the contrary. Let us recall Randi's original announcement of Beta. His statement then was: "If those who were caught in this net [Alpha] will realize their errors and adopt stringent standards of procedure, Project Beta--which is already underway--will fail" (Randi, 1983a). This statement alone should make it clear that Beta was not as later claimed. (1) Beta was supposedly set up to test the reaction by parapsychologists to Alpha, to see if they learned their lesson from Alpha. Yet, here Randi told us Beta was "already underway" prior to the revelation of Alpha. (2) Any normal reader of this sentence--and not just a "jittery parapsychologist"--would interpret this to mean that adopting proper controls, presumably in some experiment, would thwart Beta, just as Alpha would have been thwarted if proper controls had been adopted. I believe Beta then did refer to another test similar to Alpha, and this would explain the language used. (3) Note that Randi says that stringent controls (such as using the help of a magician) would result in the failure of Beta. Yet when the parapsychologists later sought the help of conjurors, Randi said that Beta was a success!

There is additional strong evidence that Beta originally meant another trap similar to that of Alpha. In Randi's "Advisory Notice Number Two" (which he says he sent out to parapsychologists "48 hours in advance of a formal press announcement" even though it is dated January 28, 1983, the same date on Randi's press announcement), he closed with the words "caveat legens--Project Beta is already underway..." (Randi, 1983a, p. 3). Clearly another warning. Yet, it should be obvious that if the psi researchers had failed to seek the help of magicians, had "failed" Beta, they would have had nothing to be warned about. What would have happened except that Randi might have said he was disappointed that they had not learned their lesson from Alpha. He surely must have meant something else. Additional evidence is to be found in the demonstrable fact that Randi made several early references to Beta in letters he wrote to persons who showed me those letters, and there he darkly hinted that particular psi researchers were potentially involved in his Beta trap. Finally, my several early conversations with Randi in which Beta was mentioned clearly left me with the impression that Project Beta was a new trap Randi had set for some experimenters.

What evidence has Randi offered us that Beta was originally what he later claimed it was in August, eight months after he announced its existence? Randi points out that he wrote a description of the nature of Beta to my neutral colleague Piet Hein Hoebens in April of 1983, but that was done several months after he first announced Beta. Critics like Randi frequently complain when parapsychologists fail to post their experimental expectations



in advance, raising the suspicion that their confirmed hypotheses might have been created after the results were in. Yet here is Randi, making a dubious claim about the nature of Beta, and yet expecting us to accept his claim without the simple proof we might have expected of him, i.e., some description of Beta pre-posted with some neutral party. Hoebens would have been quite acceptable to me, but it would have been necessary for Randi to post it with him at or before the time Beta was first announced, not several months later. Would Randi let a psi researcher get away with something like this in a purportedly "legitimate, serious experiment"?

What then really happened to Beta? It seems to me that the most parsimonious explanation is that what Randi now calls Project Gamma was probably his original Project Beta. Clearly, Randi tells us, something went wrong with his recently disclosed Project Gamma after it was initiated, and he is uncertain as to whether or not it will ever get completed. This sort of switch is rather a standard magician's ploy, the use of multiple end points. Since Randi did not initially tell us what Beta was, he could then claim any outcome he wished as representing Beta. Randi knew from me that I had been seeking to get the PA to make liaison with the magical organizations for nearly two years. He was invited by me to be on the PA panel of magicians I arranged. And he knew that many prominent magicians, including those on the panel, were critical of his ethics in regard to Project Alpha (something Randi neglects to mention in his writings). Randi knew all this prior to his letter to Hoebens about Beta. So, I offer you the conjecture that Randi may have taken the opportunity to rename his dud Beta project "Gamma" and replace it with a newly defined Project Beta which he could call successful. Randi could thus partially mend his fences and even take some credit for the PA's new appeal for help from magicians (which, by the way, had been initiated by me quite independently of Alpha and which appeal actually excluded Randi since he is not a member of the conjuring organizations addressed by the PA).

I must emphasize that although I personally believe—based on the arguments and evidence cited above—that Randi pulled a switch to make his Project Beta appear a success, I do not expect all to be convinced by my case which rests mainly on parsimony. I therefore insist that we treat my conjecture about the switch as plausible and likely rather than as proved. On the other hand, we must remember that it is Randi who has put forward the claim for a successful Project Beta, so he must bear the burden of proof for that claim if he asserts it as a continuing part of his serious scientific efforts. He may yet present us with arguments and evidence to remove what appear to be obstacles to our acceptance of his assertions, but until he choose to present such, we must remain skeptical.

#### Randi Gets Counter-Boaxed

Not all psi researchers were put on the defensive by Alpha. Dennis Stillings, director of a Minneapolis group called the Archaeus Project, which puts out a newsletter by that name, was outraged and initiated a retaliatory hoax which started as a small joke but escalated into something more significant. Stillings felt that Randi was trying to reap advantage from lies told to the psi researchers and was, in effect, blaming the victims. Stillings believed that any person could be deceived by lies and that Randi was just as susceptible to such human error as anyone. So, Stillings (1983a) issued a phony, one page, special issue of his group's newsletter (of which only two copies were

mailed out and these to Edwards and Shaw with the expectation that they would show it to Randi). The ersatz issue contained a short, two paragraph, fraudulent announcement that the Archaeus Project had just been given "a fund of \$217,000...as seed money for a program in PK research and education." It said the funds were for "grant money to PK investigators, especially those interested in 'metal bending'" and for "developing a program of educating children in the range and nature of parapsychological phenomena." Finally, it said that "Those applying for grants, as well as those gifted with paranormal abilities" should write to Stillings. Stillings also separately wrote a letter to Shaw telling him that since Shaw was a fraud, he should not apply for any of the money. To stretch the joke even further, Stillings also published a warning "Advisory Notice" (Krueger, 1983)—to parallel Randi's similar advisory notes—in a previous real issue of his group's newsletter.

Though Stillings' original prank struck me as being a bit silly (after all, Randi never claimed to be immune to trickery, and conjurers fool one another all the time), what happened next went far beyond Stillings' expectations and turned the matter into a significant episode. Upon seeing the phony announcement, and apparently without properly checking things out, Randi decided to give one of his annual psi-mocking "Uri Awards" to this receipt of a phony grant. Thus, on April 1, 1983, Randi's Discover news release gave a "Uri" in the funding category: "To the Medtronics Corporation of Minneapolis, who gave \$250,000 to a Mr. Stillings of that city to fund the Archaeus Project, devoted to observing people who bend spoons at parties. Mr. Stillings then offered financial assistance to a prominent young spoon-bender who turned out to be one of the masquerading magicians of Project Alpha—a confessed fake." In this incredible award statement, Randi managed to falsely identify a major corporation as the funding source (when no source was ever mentioned in the original announcement), escalated the award from \$217,000 to \$250,000, misdescribed the purpose of the phony award, and falsely claimed one of his associates had been offered funds!

Stillings and other foes of Randi, particularly Walter Uphoff, had a field day with Randi's big blunder. With headlines in psi publications like "'Non-Magician' Fools Conjurer" (New Frontiers Center Newsletter) and "Researcher Fools Randi Into Making Fictional Award" (Psychic News), the "Amazing" Randi was portrayed as merely "Amusing." Randi, however, was apparently not amused. He has thus far not publicly acknowledged his mistake, although he did write an apology to Medtronics and admitted his mistake in private correspondence (including a letter sent to Stillings which Stillings managed to get Randi to write him by posing as a third party). In fact, when his Uri Award list was reproduced in The Skeptical Inquirer, Randi's award to Medtronics was simply omitted without comment. Although Stillings had only intended his prank to demonstrate that Randi, too, could be fooled, it actually ended up displaying the fact that Randi is capable of gross distortion of facts and in this case, at least, shot from the hip (and here managed to hit his own foot). This naturally might lead some to question Randi's reporting accuracy in the past and should caution us to look more carefully at the past cries of "foul" that opponents have hurled at him.

#### The Soup Thickens

The aftermath of Alpha has been full of much acrimony, and there have been charges of distortion by Randi hurled at him from many quarters, especi-

ally by those like Prof. Walter Uphoff discussed by Randi in the second part of his article in The Skeptical Inquirer (Randi, 1983e). I will not take up those matters here, for the story is long and not really central to this essay. Interested readers can simply go to The Skeptical Inquirer [Box 229 Central Park Station; Buffalo, NY 14215], the New Frontiers Center Newsletter [Fellowship Farm; Rt. 1; Oregon, WI 53575], and the Archaeus Project Newsletter [800 S.E. 4th St.; Minneapolis, MN 55414] to read the charges being made and can evaluate these matters for themselves. I view those issues as peripheral ones to my concerns here and make no judgements about them now. In justice to Randi, however, it must be noted that the ultimate positive contribution of Alpha may well be found in his debunking efforts in these other areas of what parapsychologist Martin Johnson has called "para-pornography." Certainly, the claims found in places like The National Enquirer about publicized "psychics" like Masuaki Kiyota (championed by Prof. Uphoff and others) and the debunking claims about such people by Randi deserve attention and examination. That is merely outside the scope of this article. Instead, let me turn to two relevant events I learned about only after Alpha had run its course.

Randi's primary "attack" with Alpha was on the MacDonnell Laboratory. Because of the large bequest setting up that lab (\$500,000 over five years), Randi apparently thought this was a major operation and an important target. Of course, this grant gave the Mac Lab only an average of \$100,000 per year, and a good portion was presumably taken by Washington University for the overhead costs universities routinely take for administering grants. So, the Mac Lab has never been the best funded psi research operation in this country, as one might misassume from some press reports.

In addition to Randi's negative "Uri Awards," he also gives what he terms "Straight Spoon Awards" which commend researchers for their positive contributions. When Randi made his first press announcement about Alpha at Discover, in 1983, in which he criticized Prof. Phillips and the Mac Lab for their credulity, he failed to mention something important: On April 1, 1982, less than a year before, Randi had awarded Phillips his "Straight Spoon Award" because Phillips had reconsidered his position about using only loose controls on Shaw and Edwards and decided to re-design his experimental procedures after what Randi called a "less-than-enthusiastic reception for his presentation at the 1981 meetings of the Parapsychological Association" (Randi, 1982; Potash, 1982). One would have thought Randi would mention this award in his writings about Alpha, but that would have made Phillips look less foolish when Alpha was announced. Since Randi told us that Alpha "was not intended to embarrass or belittle any person" (Randi, 1983a), one might have expected him at least to mention this "honor" bestowed by him upon Phillips in the very midst of Alpha.

A second curious matter emerged in a letter sent in to The Skeptical Inquirer (Chalmers, 1983-4) calling attention to the "unnoticed irony" in the fact that the journal had earlier published an article (McBurney and Greenberg, 1980) in which Steve Shaw, who had been posing as a real psychic long before Alpha (e.g., cf., Anonymous, 1977; Hazlett, 1979) was purportedly unmasked as a fraud. It seemed that no one had noticed this. The reply to Chalmers' letter pointing this out stated that Randi claimed that Shaw's presentation of himself as a genuine psychic was part of Shaw's "cover" (Frazier, 1983-4), but it seems most surprising that Randi never brought this up in his revelations about Alpha. After all, he could have pointed out how those

who endorsed Shaw's PK powers stupidly overlooked this clear warning to them that Shaw was a phony. It seems most likely to me (again, I argue from parsimony) that Randi himself may not have realized this article appeared in his own organization's journal until Alpha was well underway. Randi informed the Mac Lab about this article in late December of 1981, and Phillips replied about it to Randi the next month, pointing out that the alleged debunking was actually of little force since it did not really unmask anything but only proved that Shaw failed to demonstrate psi when controls had been instituted; he had not been caught cheating at all. Perhaps Randi found Phillips' argument on this convincing, or perhaps he did not want to call attention to the fact that Shaw had been presenting himself as a real psychic long before the initiation of Alpha (cf., Anonymous, 1977 and 1978). (The fact that Shaw had been posing as a genuine psychic since 1977, long before he had even met Randi, negates the claim that he was doing so merely as part of his "cover" for Alpha.) Otherwise, why did Randi refrain from mentioning this matter when he brought in far less impressive things to demonstrate the supposed credulity of psi researchers? Until Randi gives us a formal and full accounting of Alpha, or makes publicly available the "388 documents" which he said "tells the story of the Project" (Randi, 1983a), this odd episode probably will remain a minor mystery.

#### What Can We Conclude?

The Alpha story has not yet ended. We may yet hear more about Project Gamma, and Randi may even be working his way slowly through the Greek alphabet. This essay was not intended to present answers so much as it is meant to explore the facts and raise questions. I may have misevaluated some matters and unintentionally misrepresented others. I sincerely hope that Randi will respond to this essay and correct any factual errors. He is most welcome to space in this journal to criticize my analysis. I still believe that Randi frequently acts as an admirable critic. Like the "little girl with the little curl," when Randi is good, he can be "very, very good." On balance, I still believe his presence in the paranormal debate has been healthy. He has done some very competent debunking in the past, and despite what I have conjectured about his motives during the Alpha case, I think Randi believes himself an honest antagonist. Like all of us, Randi is human and is capable of error; and since he aspires to deal simply with complex matters, sometimes his errors have been large. Frankly, I hold Randi far less responsible for his mistakes and zeal than I do those supposedly serious scientist critics who accept his pronouncements uncritically.

First and foremost, we all must remember that Randi is a professional conjuror. He is not a trained scientist although he is remarkably well versed in many areas of knowledge. He is, I think, what Ray Hyman (1980) termed a "hit man" brought in by scientists to discredit (rather than disprove) unorthodox and extraordinary claims. It is a great mistake to think that the norms surrounding a conjuror create the same obligations which define the scientist (cf., Collins, 1983). Like his fellow non-scientist debunker Martin Gardner (1981, p. xvi), Randi is often more interested in seeing his opponents defeated by horselaughs than syllogisms. Randi has said that he hoped Project Alpha would fail due to the vigilance of the researchers tested. Does anyone seriously believe Randi wanted that to be the result? (As Randi might say, "If you believe that, I have some swamp land I'd like to sell you!") Randi is in effect a kind of vigilante, one outside the normal sanctioning order of sci-

ence, one who can violate the regular rules in his search for a more efficient "higher justice." Thus, he is more like a fabled gunslinger than an official law man; more like Mickey Spillane's Mike Hammer than a district attorney constrained by problems like due process and civil liberties. He is not obligated to observe the normal collegial courtesies within the scientific community. If a credentialed parapsychologist discovered a colleague conducting poor or even fraudulent experiments, he would be expected to bring this matter to the attention of the Parapsychological Association or publish his complaints in a peer-refereed journal. A scientist is normally expected first to complain about his peers' bad work to his colleagues, who are expected to investigate matter dispassionately, before going directly to the public via the mass media. A normal scientist would be criticized—even if his facts were true—if he first went to a major popular science magazine and held a press conference at which he announced his revelations and told us that details would soon appear on his forthcoming TV show. (Note that Randi would similarly criticize a psychical researcher who ran to the media with a popular account of his work before first having it published in a proper technical/scientific forum. But such actions are understandable—and I would argue somewhat excusable—in the case of a professional magician whose livelihood depends not on a tenured university position but upon media publicity. (Since many parapsychologists, even in the universities, do not have tenured positions and are less likely to get it because of the sort of publicity Randi brings their field, they probably would not agree with me that Randi's actions have been at all excusable.) It is the job of a conjuror to get publicity, and Randi is damned good at that job. Even conjurors who have been at odds with Randi (and many, including Walter Gibson, publicly voiced their disapproval of Project Alpha) have expressed admiration for his ability to get great media coverage.

The simple fact is that Randi is behaving much like other conjurors have behaved in the past, including Harry Houdini. It should be expected that he would seek to elevate the status of his activities into that of a noble champion of science against pseudoscience. Would we expect him to say, "Aw shucks, take my scientific efforts with a grain of salt; I'm just a magician"? Given everything involved in the difficult life of a performer, I personally think Randi does remarkably well by the truth as he sees it. This in part is because Randi seems really to believe his own rhetoric about his noble fight against the dark forces of irrationality and pseudoscience that threaten Western Civilization. He is not the blind fanatic some suppose, but he is a zealot, and I believe he means well (though G.B. Shaw reminded us all that "The road to Hell is paved with good intentions"),

In my view, the problem is that too many scientists have uncritically accepted Randi not only as their knight but have given him quasi-scientific authority. It is no coincidence that Randi mentions the CSICOP every chance he gets and has it prominently on his stationery; for while plugging his group, he also reminds us of the scientific company that legitimates his activities and elevates them to "scientific inquiry" rather than "publicity seeking." So, to paraphrase Shakespeare, "The fault, Dear Brutus, is not in this Star but in ourselves." We simply never should have taken Randi for more than he really is: an excellent magician who can perform a most useful role as an expert witness in the adjudication process of science. We should never have elevated him to the role of a major advocate or lawyer in the court of science, and we certainly should never treat him as either judge or jury. Yet this is what has happened. For example, Randi was designated as the spokesman, rather than as an important expert witness, when he recently was invited to present the case

against parapsychology on a panel at the 1984 meeting of the American Association for the Advancement of Science. Many parapsychologists indicated they felt this was rather an insult to parapsychology since they would have expected a scientist to be their critic on such an occasion. We really can not blame Randi for accepting such an "honor," but we should be critical of those who accept him in such a role. (This has nothing to do with how well Randi performed on that platform.) In short, I blame the scientific community -- especially the science publications like *Discover*--and not Randi for the uncritical reactions to his escapades, including Project Alpha. In my view, Randi is just doing his job and doing it well; but, it seems to me, too many otherwise critical scientists and science writers have failed in their jobs because they were so amused by Alpha that they failed to notice the damage to fairness and truth that took place amidst our laughter.

\* \* \* \* \*

#### Epilogue: On Alpha and Me

Because Martin Gardner (1983 and 1983-4b) has published a number of false statements about my own connection with Alpha, I will take this occasion to set the record straight. I first learned of Alpha in late July of 1981 via a conjuror whom Randi had proudly told about the project. Thus, I did not learn of it from Randi, and I immediately wrote him a letter (which I also sent to several mutual friends in the conjuring fraternity) telling Randi that I had learned of it and had deep misgivings about how he might handle the affair. Randi promptly replied by phone and initially flatly denied the existence of the project. It was only when I told him I had confirmed the story via a mutual conjuror friend (not my original source) that Randi admitted Alpha's existence and complained that our mutual friend had even confirmed the story for me.

I pointed out to Randi that I was concerned with the likelihood that he might exaggerate the results of his "experiment," overgeneralize the results to those researchers in parapsychology who were not incompetent, and use the project more as a publicity device that could hurt people than as a constructive effort to help psi researchers do better science. Randi assured me that his intentions were the best, that he actually hoped the researchers would avoid the trap, and expressed his conviction that in the end the parapsychologists themselves would be grateful to him. I was impressed by Randi's apparent awareness of the deeper issues involved. I had been friendly with Randi for some years, both as a colleague interested in matters paranormal and as a fellow conjuror. I trusted Randi. However, contrary to what Gardner has written (1983-4b), I never gave Randi my word that I would not reveal his hoax. Since Randi did not originally confide in me about the hoax--and even tried to deny it to me at first--I felt under no obligation to Randi. In my letter of July 29, 1981, to Randi about Alpha I wrote: "I do not plan to do anything about all this until at least a week before the PA meetings--if I do anything at all. But I must try to balance the likely good your operation will do against the harm I think it likely to do. The more information I have, the easier any decision I make should be. I am not at all anxious to blow the whistle on this scheme. You seem to have invested much in it and it has--as I have noted--a positive side. But if you can persuade me that there is less to the negative side (or more to the positive side), then I would feel far more comfortable in not warning those I consider the likely innocent victims of your plot." I told Randi nothing thereafter that would indicate any change in

my position on this.

Perhaps the most important factor which induced me not to inform the researchers being hoaxed was that I thought Alpha, if properly done, would likely produce very important results. Randi expressed his hope that Shaw and Edwards might move beyond the Mac Lab and have their phoney PK abilities "validated" by a major British researcher and/or others who had previously published reports about young subjects with similar purported powers. If Randi could actually accomplish this, his accomplices would become a control group against which we could measure the competence of these same researchers who had issued reports claiming the validation of PK powers by others. As such, it seemed to me, Alpha might truly become a "legitimate, serious sociological experiment." Given Randi's assurances—which he repeated many times to me over the next year and a half, on one occasion in a small group discussion we had with two other people who knew of his hoax—that he would act responsibly and carefully so that we would all be proud of his effort, I told Randi that I had no plans to communicate the secret of Alpha to those at the Mac Lab.

Though I did not communicate the secret of Alpha to any of Randi's victims, I did confidentially inform a few key persons, especially several science journalists, including two at Science about the matter; and I kept them informed of Alpha's progress to the limited degree that I was aware of what was happening. Perhaps this was a mistake on my part. I realized that there was the chance that matters might thus leak out, but I felt this small risk had to be taken. So far as I have been able to ascertain, these confidences were kept. I did this mainly to keep Randi from being the only source of information about Alpha when he finally broke the story. I did not want these journalists to receive only Randi's version of the events. I realized there was the danger that he might turn it into more of a media event than a useful scientific exercise, and I had from the beginning feared that Randi might exaggerate the implications of Alpha for the rest of parapsychology. Since he ended up doing precisely that, I have no regrets about the outcome from my disclosures.

In retrospect, it is easy to say that I might better have done this or that. I remain uncertain about whether I took the wisest course of action. Perhaps I should have immediately informed the Parapsychological Association; perhaps I should have remained entirely silent; or perhaps there were other alternatives I might have taken. Given the complexity of the issues involved and my own conflicting feelings, I took what then seemed to me the most prudent course. Upon reviewing my own actions, I can only assert that I did not act hastily nor did I act with malice. Nothing I did, so far as I have been able to ascertain, hurt anyone or had any significant effect on the progress or outcome of Project Alpha. I broke no promises and revealed nothing told to me in confidence. I can only ask that if others judge my actions, they do so based on the actual facts and not on misinformation, misrepresentation or rumor.

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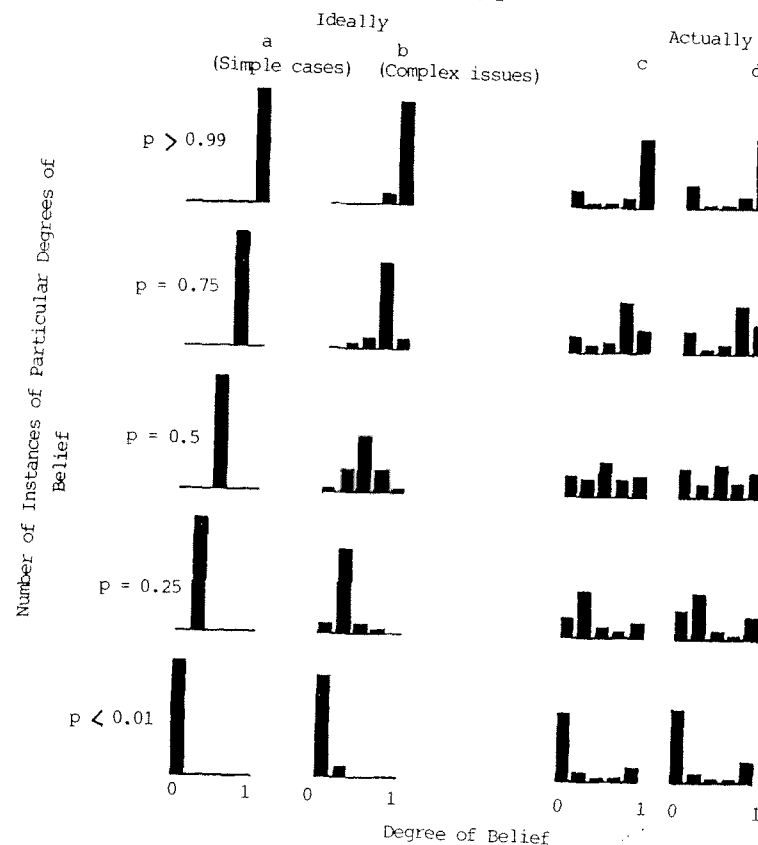


## DISTRIBUTIONS OF BELIEFS ON CONTROVERSIAL MATTERS

Henry H. Bauer

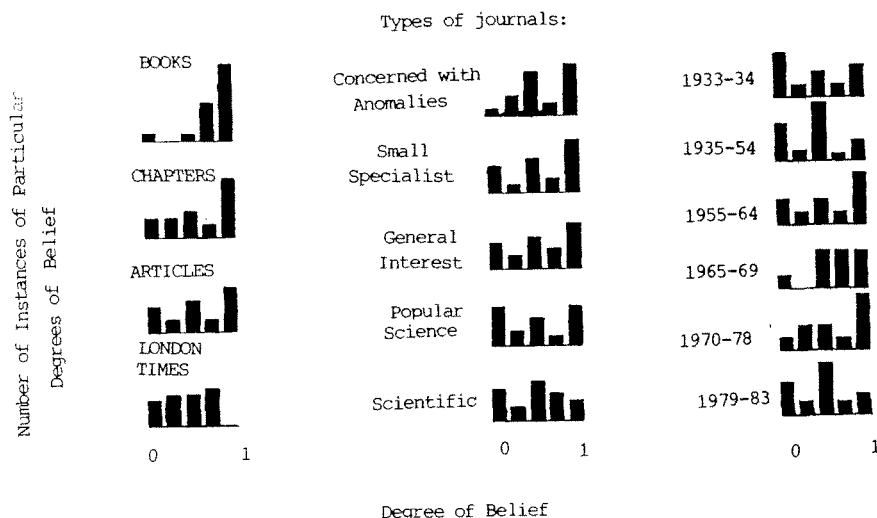
Subjective probabilities do not always correspond to the facts. Particularly when the facts are not readily established, arguments therefore ensue. I suggest that controversies might be usefully categorized the manner in which degrees of belief (i.e. subjective estimates of probability) are distributed; particularly by looking at how these distributions vary among different groups of people (e.g. laymen and experts), in the relevant literature, and over time.

FIGURE 1



Ideally, subjective probabilities would correspond to reality and yield belief distributions as in Figure 1a (simple situations) or Figure 1b (complicated matters). On such straightforward issues as the

FIGURE 2  
Belief Distribution in Literature about Loch Ness



throwing of a die, for example, the actual probability is known and also easily calculated: so (Figure 1a), unanimity will prevail on the view that a situation with probability 0.5 indeed has a probability of 0.5, and people will be prepared to lay even odds on the event. On more complicated matters, rational people can differ over the postulates and assumptions involved -- for example, in calculating the probability that extraterrestrial intelligence exists. Then there will not be a unanimous view but a distribution of beliefs (Figure 1b), ideally a normal distribution about the actual probability.

But human beings do not judge probabilities dispassionately on the basis of the best evidence, nor is that best evidence widely or readily available (particularly not on controversial matters). Our judgements are influenced by preconceptions, and by the desire to be certain rather than agnostic, and by emotional involvements (to save face, to exert authority, and so on): in consequence, beliefs tend to be polarized to the two extremes and not distributed in a Gaussian manner. Further, controversies are muddled by ignorance, chicanery, and a prevalence of misinformation, which would tend to smear out the distributions of beliefs (it is somewhat chancy whether one is exposed to good information or to misleading data either too positive or too negative). Actually observed distributions of belief would therefore differ from those in Figure 1a or 1b as a result of polarization and broadening to resemble Figure 1c (somewhat polarized opinions) or Figure 1d (yet more polarized).

When analyzing<sup>1</sup> what has been written on the controversial matter of the Loch Ness monster, I found such belief distributions quite illuminating. Figure 2a shows degrees of belief expressed in books, in chapters or sections of books, in journals and magazines, and in a newspaper. Books express predominantly a believer's viewpoint; neutral or debunking works are little in evidence. That corresponds to intuition -- how often it is said that books of mysteries and anomalous claims sell well and are readily publishable whereas skeptical works are difficult to market. But concerning Loch Ness, chapters of books display a much wider distribution of degrees of belief: that is not necessarily intuitively to be expected; nor that articles in periodicals show a much greater degree of polarization of beliefs. That the London Times skews toward disbelief, however, might well have been expected by anyone who is even slightly familiar with newspaper coverage of Loch Ness (and of similar topics).

To understand the high polarization evidenced in periodicals, examination by type of periodical seemed an obvious necessity; and some clear differences emerged (Figure 2b). Magazines specially concerned with anomalous claims were polarized strongly, as between neutral and strongly inclined to belief: that is plausibly explained by the preponderance of two sorts of articles -- pure reporting of alleged sightings ("neutral" as to expressed belief that an anomalous animal is involved), and interpretive articles overwhelmingly favoring the reality of Nessies. I found interesting that pieces in such scientific journals as *Nature*, on the other hand, showed something almost akin to a Gaussian distribution of belief, whereas articles in journals of popular science were again more polarized: perhaps writings for a scientific audience are more cautious and less dogmatic than those intended to expound science to a wider audience (not a particularly novel or startling suggestion, I admit). But it is not my purpose here to claim new insights from these results, nor definite explanations of them: rather to claim that looking at such controversies in this fashion can illuminate them; further, that we could understand a great deal more if similar data were available, for comparison, about other controversies.

I found particularly interesting the change in belief distribution over time, evidenced in the magazines (Figure 2c). Belief became more prevalent after 1955, and during the late 1960s there was almost a consensus in this literature that Nessies were real; belief remained high in the 1970s though opinions were again polarized. Most recently, however, the degree of belief has decreased and the degree of polarization increased again: the distribution is very like that of 1935-54. Those years were ones in which almost no new evidence or claims about Loch Ness were made public; and it is the case that once again, very little new evidence has been advanced in the most recent years, indeed not since the underwater photographs of 1972 and 1975. Perhaps, then, these belief distributions do reflect happenings in the controversy in a usefully accurate and explicable manner.

I suggest also that belief distributions could serve to characterize different sorts of controversies. The beliefs of experts and of laymen would differ markedly on anomalies, I expect; whereas on more socially



and politically sensitive subjects -- nuclear power, anti-missiles -- I would expect the beliefs of experts and of laymen to be more similar. (On the first sorts of issues, evidence might be more influential than emotionally charged preconceptions; on the latter issues, political and social prejudices are paramount.) More specifically:

1. Controversies within science: there is general agreement on the nature of the issues and of the evidence, whose influence outweighs non-cognitive factors; belief distributions among scientists approximate Gaussian; the controversies have no immediate political implication, so the belief distributions expressed in the media mirror those of the scientists; the distributions of the population as a whole mirror those projected in the media.
2. Anomalous claims: the experts' beliefs, strongly influenced by what is known and by non-cognitive factors, are strongly skewed to disbelief with great unanimity (little polarization); the population as a whole is strongly polarized, with belief generally predominating; the media are in between and there are differences according to the type of newspaper or journal; the literature as a whole is highly polarized.
3. Technical matters of sociopolitical importance: there is strong polarization; beliefs are determined more by ideology than by particulars (see Weinberg<sup>1</sup>); differences between belief distributions of technical experts, media, and the population as a whole are minor.

Whether these particular expectations are sound or not, the possibility exists that controversies could be segregated into several categories through studies of belief distributions, and such classification could lead to further insights.

#### NOTES:

- <sup>1</sup>Henry H. Bauer, "Society and Scientific Anomalies," Journal of Scientific Exploration, in press.
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# A CONSTRUCTIVELY RATIONAL APPROACH TO PARAPSYCHOLOGY AND SCIENTIFIC METHODOLOGY (RESPONSES TO MY COMMENTATORS AND ATTEMPTS AT SOME FURTHER CLARIFICATION)

Gerd H. Hövelmann

*"Critics who prefer to reserve judgment on the claims of parapsychology are not entirely irrational."*

D.J. West, "parapsychologist"<sup>1</sup>

*"There are influential parapsychologists who ... have successfully resisted the temptations of unreason."*

P.H. Hoebens, "critic"<sup>2</sup>

## I. INTRODUCTORY REMARKS

It always means a double-edged enterprise if a philosopher of science interferes with discussions within a special branch of science, and all the more so if, with normative intent, he dares to make recommendations as to how members of this special branch of science ought to adjust their practice, present their research findings, or build their theories. The situation is complicated further if this philosopher has himself, in one way or the other, dabbled in this particular field of research and -- at least equally important -- if it is not yet established whether the field under consideration can at all be justly viewed as a legitimate branch of science.<sup>3</sup> All of these unpleasant difficulties apply conjunctively to my original paper and are reflected in the dialogue around it. Such difficulties may lead (and, in fact, have led in some instances) to misconceptions of various kinds; thus, some of my commentators apparently had considerable problems assessing the position I might be taking in debates on matters parapsychological (the fact that I approached the field of parapsychology only a few years ago may have contributed to this confusion). Since comments on a scientific paper frequently are not completely independent of the (previous) assessment of the author's general position on the issues in question, these problems have inevitably led a few commentators to fail to fully comprehend the main purpose of my article. Finally, some other misconceptions are probably due to the fact that, apparently, I did not succeed in presenting my arguments in a way as clear and intelligible to anyone as would have been desirable.

So far (May 13, 1983) my recommendations have provoked almost two dozen commentaries which are of an amazing variety. In order to correct some of the old misconceptions as they are evident in a few of these commentaries and to prevent occurrence of new ones, it will be unavoidable to clarify my general position on some of the matters at issue along with my responses. Wherever possible, I will do that rather briefly in my responses to single commentators in section II; since some other clarifications require more space, however, I have decided to treat a few of the more important questions in another section (III) following my individual responses. There I will explain my thoughts on

(what is wrongly called) the Kuhnian "philosophy" of science and try to point out why parapsychologists should not adopt it (III.1); I will briefly deal with the question as to how to construct a scientific terminology survival problem (III.3). Furthermore, I will try to explain (especially in my responses to Dr. Stokes and Professor Zusne) my personal attitudes towards the legitimacy of parapsychology and the legitimacy of criticism, and I will have something to say on the mutual relations between the parapsychological camp and that of the critics. This proceeding will, moreover, cause the welcome side-effect that the occurrence of annoying overlappings can be avoided to a certain degree. Overlapping arguments put forward in several of the commentaries will each be treated in detail in one individual response, and reference will be made to that respective treatment when a similar argument turns up in another commentary.

What I wrote about occasional misconceptions above was not to say that I feel largely misunderstood by my commentators. On the contrary, I think that, though a few commentators seem to have missed some crucial points, a considerable number of most pertinent and thoughtful comments, questions, and objections have been put forward, and I noticed some vague but promising signs indicating that the whole enterprise might not have been in vain. Only the future will show, however, whether I am right about that.

To conclude these introductory remarks, I thank the many knowledgeable persons who spared time and effort to participate in this zetetic dialogue on some recommendations for the improvement of parapsychology's practice.

## II. RESPONSES TO MY COMMENTATORS

### Response to John Beloff

In the seventh recommendation of my paper, I urged parapsychologists to lend their support to Dr. Beloff's suggestion for an official Commission of Enquiry. Seven out of twenty-three commentators dealt with this proposal in their remarks on my recommendations (besides Dr. Beloff, these include Mr. Hoebens, Professor Leeds, Dr. von Lucadou, Dr. Nash, Dr. Palmer, and Dr. Stokes), and all but Dr. Beloff himself declared themselves more or less explicitly against this suggestion. The main argument was that previous experiences with similar commissions suggest that they are unlikely to resolve anything if (as in parapsychology) the problems at issue are controversial. While these six "negative" votes certainly do not form a representative sample of parapsychologists' opinions on the desirability of such a Commission of Enquiry, the uniformity and unanimity of these comments may well justify the assumption that Dr. Beloff's suggestion would have only little chances of being accepted by a larger group of parapsychologists, say, by the members of the Parapsychological Association. Nevertheless, I continue to believe that some kind of "concerted action" might prove to be very useful for solving some of the problems we confront.

Dr. Beloff fears that if my recommendations should turn out to miss their desired effect, "we may find that we have sacrificed a large slice of what has traditionally constituted the subject-matter of our science to no purpose." Turning this argument around, he seems to be suggesting that we should carry on investigations in some areas for the mere reason that they have traditionally "constituted the subject-matter of our science." I am

not prepared, I confess, to share this opinion. I rather think that there are indeed some such "slices" which could well be "sacrificed" without parapsychology being any the worse for it.

Dr. Beloff then holds that my recommendations "embody serious misconceptions" and inadequacies. I will deal with these alleged misconceptions in a few moments, but first I should try to point out an obvious misconception on Dr. Beloff's part: In the passage I quoted above, he speaks of the "desired effect" of my recommendations. There seem to be some confusions as to what this "desired effect" was. Dr. Beloff appears to believe that my recommendations were mainly intended to provide parapsychologists with guidelines or tactical tricks for a better window display, to enable them to "placate our scientific neighbours" or to chum up more effectively to the scientific community "in the interest of some supposed respectability" (some other commentators, such as Dr. Scott, in particular, seem to share this impression). Dr. Beloff is dead wrong, however. Suppose that parapsychology in its present constitution already were a well-recognized branch of science, that parapsychological departments were firmly established at every other university, that leading scientific periodicals, such as *Science*, *Nature*, *New Scientist*, or *Scientific American*, were regularly devoting considerable parts of their space to "insiders" reports on parapsychological research, that this research were sufficiently funded, that there were no critical opposition whatsoever against parapsychological research, and that James Randi were studiously organizing sightseeing tours to the major parapsychological research centers; given all this, my recommendations would have been almost entirely the same! In the first place, my paper was concerned with the methodical rigidity of actions in parapsychology, with the criteria for the validity of scientific propositions, and with the question as to what forms conclusive evidence in science. And my main claim was that parapsychology will hardly be accepted as a legitimate branch of science unless we conform to the mores of sound and rational scientific conduct<sup>4</sup>.

To turn now to my alleged misconceptions, Dr. Beloff claims that I ignore that theories or hypotheses never "exclude any alternative explanation" (my underlining). Of course, they don't. A possible alternative explanation to Ptolemy's theory of epicycles (which can be traced back to Appollonius and Hipparchos and which was replaced after the advent of the heliocentric conception) would have been, for instance, that a deity forces the planets to move uniformly in circular orbits around particular points (the centers of the planets' epicycles) which, on their part, are forced to move in circular orbits (the "deferents") around earth. The crucial question here is: What do we accept as an explanation? I would propose that we call a phenomenon "explained" if know how to produce<sup>5</sup> it. The notion: "The explanatory value of theory A is higher than that of theory B" can then be reconstructed to read: "Relative to the attainability of a given purpose, theory A permits of the performance of more purposeful actions than does theory B." This is the basic principle, I think, by which scientists, in fact (whatever they may be telling us), decide on acceptance and rejection of (alternative) explanations<sup>6</sup>. (So, the proof of the pudding does not lie in eating it but rather in knowing the recipe). In terms of this principle, I think, there are a lot of reasonable alternative explanations especially in the case of the debatable survival hypothesis or in case of spontaneous paranormal occurrences.

Now, Dr. Beloff further complains that what I have said about the evidential value of spontaneous cases (my recommendation 3) "means, in effect, that no anecdotal evidence, however carefully researched or corroborated, is worth anything as evidence for what actually transpired. One wonders whether the author has stopped to consider that if this assertion were generally conceded it would become virtually impossible to convict anyone of anything in a court of law." He is quite right in assuming that, in my opinion, spontaneous cases are not "worth anything as evidence" in a scientific sense. This is simply because the conditions under which these cases occur are not under our control. In my view, "anecdotal evidence" is contradictory in terms. Appealing to legal proceedings is of little help here. The crucial work in the quotation from Dr. Beloff's comments is the work "generally." Remember what H. Arthur Smith, himself a lawyer by profession, said in his presidential address to the Society for Psychical Research some seventy years ago:

"My first comment must be that it is by no means clear that a lawyer's is the most important point of view in a case like that before us. The lawyer is conversant with certain canons which in the aggregate are known as the law evidence ... These canons are no doubt based on the principles of inductive logic, but in the application of these principles they are cribbed and confined on all sides by the exigencies of forensic practice. It is far from being certain that the dialectic methods of the forum are the methods best adapted for the investigation of questions not subject to similar restrictions, but carried on under entirely different conditions" (my underlinings).

And Smith adds:

"... though there is much evidence quod debet monstari, that is, what lawyers call evidence to go the the jury, there is absolutely nothing that can be called evidence quod facit videre (Professor [William] James has somewhere called it 'knock-down evidence') from a lawyer's point of view; nor can I conceive that there is ever likely to be" (Smith's underlinings).

This assessment speaks for itself; one of the main problems is, moreover, that the "principles of inductive logic" are quite insufficient to help to prove anything in the sense of empirical science<sup>7</sup>. So, the question should not be whether we "have nothing but the highest respect for" certain scholars, but whether we find their interpretations reasonable and justifiable. Furthermore, it has apparently escaped Dr. Beloff's attention that I did not recommend to "renounce ... all investigation involving spontaneous real-life incidents"; I merely urged parapsychologists not to ascribe any evidential value to them.

Then Dr. Beloff shifts his ground, emphasizing that he believes that parapsychologists are dealing with "the world of mind rather than the world of matter" (my personal opinion is, however, that we are essentially putting the wrong questions here and that the so-called mind-body problem may be a "Scheinproblem," a sham problem). We should be well aware, moreover, that any effort to prove the existence of "mind" scientifically must necessarily rest on inferences drawn from material and/or behavioral changes. As far as I can see, there is no way out of this problem. This does not necessitate the claim,

however, that "everything must ultimately be explainable by the laws of physics." It merely shows that "mind research" is subject to certain methodological restrictions. Therefore, it appears at least premature and unreasonable to speculate, as Dr. Beloff does, on the revolutionary impact parapsychology may have on "the prevailing metaphysics of materialism." In section III.2, I will try to answer Dr. Beloff's objections against my "obscure" discussion of terminology in parapsychology.

Since I have focussed disproportionately on my differences of opinion with Dr. Beloff, I may have created the impression that I generally disagree with many of the views he holds. That is definitely wrong. I greatly admire Dr. Beloff's prudence in scientific matters. The creation of such false impressions caused by the treatment of only limited parts of a certain subject may be the only disadvantage of a zetetic dialogue.

#### Response to Susan J. Blackmore

I am glad that Dr. Blackmore seems to agree with most of my recommendations. Commenting on my first recommendation, she writes: "He seems to imply that there is nothing revolutionary in parapsychology at all." That is the very point I want to make but I would even move a step further and hold that, as far as the production of scientific propositions is concerned, there is nothing revolutionary in any science at all! Unfortunately, in my own comment on my first recommendation I wrote that "some scientific endeavors may eventually lead to a fundamental change in a currently accepted basic scientific concept which ... may be described as a ... 'scientific revolution.'" I did so because I considered it out of place to discuss my objections to the Kuhnian conception of scientific developments and scientific change there. Meanwhile, it has become clear to me from many commentaries that I should have mentioned these objections to make the background of this recommendation clearer. Therefore, I have here included section III. to which I may refer readers for a justification of my view that there is nothing revolutionary in any branch of science.

I was a bit surprised by Dr. Blackmore's confession that she was "at least partly drawn into the field because of the feeling that it was challenging the accepted concepts of psychology and other sciences." It is evident to anyone familiar with Dr. Blackmore's writings, I think, that her attitude must have considerably changed in the meantime. What I would be most interested to learn, however, is if (and how many) others were attracted by the same reasons!

Dr. Blackmore writes that the question of survival after death "was the fundamental question to many of the early psychical researchers and is, I suspect, still so for many parapsychologists today" (her underlining). From a recent paper by George Zorab<sup>10</sup> it can easily be seen that the first part of this description is quite correct, while there are reasons to believe that the influence of researchers referred to in the second part is decreasing. I conceive this as an advance. I have already dealt with the question of alternative explanations in my response to Dr. Beloff; and on some basic problems associated with survival research I will be commenting in section III.3, below.

I find Dr. Blackmore's additional recommendation<sup>11</sup> extremely thoughtful and of considerable importance to the field. She is quite right pointing out that the crucial question is: What are the immediate goals of parapsychological research? One possibility is that we deal with certain hitherto

unexplained effects and immediately stop researching as soon as appropriate explanations are offered to account for these effects<sup>12</sup>. This would mean that, as long as parapsychologists are still investigating a certain phenomenon, one may be absolutely sure that an explanation of this phenomenon is not yet within sight. I would strongly object to any such attitude. Marcello Truzzi's recent statement with regard to "anomalistics," also fully applies in this case:

"If anomalistics is to be a scientific orientation, its goal must be to explain anomalies not gloat over their being unexplained. I think we must admit that many anomaly-seekers want things "unexplained" and enjoy anomalies because they are puzzles ... But puzzles for the sake of puzzles is not basically a scientific attitude. Anomalies are means to an end: improved science. They should not be an end in themselves" (my underlinings)<sup>13</sup>.

I have nothing to add to this. The second possibility pursuance of which I would strongly recommend is that we follow "every promising route wherever it leads, even if that means away from the paranormal," and that we blend in, for instance, with anomalistic psychologists<sup>14</sup> to do the necessary work to truly resolve the issues. In short, I strongly support and wholeheartedly agree with Dr. Blackmore's recommendation. I am convinced, moreover, that her argument can be applied to all of parapsychology's subject matter (she is mainly discussing the problem with respect to OBE research) since, even with subject matters changing, the basic constellation remains the same. Parapsychology may thus be described as part of an interdisciplinary approach to some hitherto unexplained effects. What else could be the goal of a science called parapsychology if not to explain the unexplained?

#### Response to Hans Jürgen Eysenck

I do not share Professor Eysenck's apparent view that our terminological problems are "inevitable" (for an exception, see section III.3) He imputes the opinions to me that some sort of "explanatory terms" were desirable, that other sciences dispose of such terms, and that parapsychology has just failed to develop such terms in its previous history. In fact, I rather think that technical terms in the sciences are to be defined in terms of the scientist's experimental actions<sup>15</sup> and in accordance with the criteria for the correct functioning of his measuring tools (at least in the natural sciences). Professor Eysenck's appeal to "the philosophical problem of causality" does not serve his own purposes here, since the term "causality" itself can only be methodically introduced by way of "elementary predications" (cf. section III.2) on the basis of the scientist's active control or production of causes and effects.

Of course, there are a lot of reasons to assume that many of the contradictory findings in parapsychological experiments are brought about the way Professor Eysenck points out. But this cannot serve as an argument, as he seems to believe, against my recommendation that parapsychologists should not prematurely give up the concept of lawfulness. From the mere existence of the mentioned contradictory experimental findings, it can not be conclusively inferred that they are constitutive of paranormal events since any such inference would have to make use of inductive logic which can be shown to be highly defective (again cf. note 9).

In commenting on my first recommendation, Professor Eysenck calls in presumed testimonies by such distinguished witnesses as Copernicus, Galilei, and Kepler. According to Professor Eysenck, "Copernicus realized that his views were revolutionary," and "Copernicus, Kepler, and Galileo claimed to be both revolutionary in their findings and theories." Now, I have briefly checked the works of Galileo, Kepler, and Copernicus<sup>16</sup>, but was unable to find any hint indicating that they might have perceived themselves as revolutionaries, and I very much doubt that they ever did. On the contrary, even Thomas Kuhn -- whom one would rather expect to support Professor Eysenck's claim -- is forced to admit:

"The principle difficulties of the *De Revolutionibus* and the ones that we may not evade arise ... from the apparent incompatibility between that text and its role in the development of astronomy. In its consequences the *De Revolutionibus* is undoubtedly a revolutionary work ... But, to any reader aware of this outcome, the *De Revolutionibus* itself must be a constant puzzle and paradox, for, measured in terms of its consequences, it is a relatively staid, sober, and unrevolutionary work. Most of the essential elements by which we know the Copernican Revolution ... are not to be found anywhere in Copernicus' work. In every respect except the earth's motion the *De Revolutionibus* seems more closely akin to the works of ancient and medieval astronomers and cosmologists than to the writings of the succeeding generations who based their work upon Copernicus' and who made explicit the radical consequences that even its author had not seen in his work ... The book gave rise to a revolution that it had scarcely enunciated. It is a revolution-making rather than a revolutionary text ... As a whole the *De Revolutionibus* stands almost entirely within an ancient astronomical and cosmological tradition"<sup>17</sup> (my underlinings).

"... Copernicus is frequently called the first modern astronomer. But ... an equally persuasive case might be made for calling him the last great Ptolemaic astronomer."<sup>18</sup>

In a very similar way, also Edgar Zilsel<sup>19</sup> and Stephen Toulmin<sup>20</sup> (both experts in the history of science) express themselves. When I mentioned these results of my literature search to Professor Eysenck, he wrote back conceding that "I had taken it for granted that this view of their activities, and their own assessment of it was widely accepted." <sup>21</sup> Since Professor Eysenck's argumentation rests almost entirely on these presumed testimonies by Copernicus et al., I remain totally unconvinced by the conclusion he draws from his discussion of my first recommendation and especially by his conclusions that "of course the views of parapsychologists are revolutionary" and that "the revolutionary role is forced on them by the very nature of their data" (also cf. section III.1). To turn once more back to Copernicus: Even if Professor Eysenck were right that Copernicus claimed to be a revolutionary, this could never be construed as a systematical argument in support of some parapsychologists' claims to be revolutionaries. If we argue in the interest of systematical clarifications, Copernicus' self-assessments are of only very limited importance, after all.

#### Response to Piet Hein Hoebens

I was especially pleased with Mr. Hoebens' introductory remarks pointing to the possible meaning my recommendations may acquire when compared to traditional German parapsychology and especially to the "Benderian Credo." Although this was by no means the main reason for writing my paper, my recommendations can well be read as a criticism of positions represented by Hans Bender and some other parapsychologists and as an expression of support for the new conservative current in German parapsychology. I deeply regret that Bender apparently decided to avoid public confrontation by not commenting on my paper, although he had repeatedly promised to write up his objections against my recommendations (so, for instance, in his letters to me of September 15 and October 18, 1982).

I would fully agree to Mr. Hoebens' first comment if a "revolutionary outlook" and the Kuhnian concept of scientific change were not inevitably entailing the problems he is branding: that is, excusing shortcomings of research in parapsychology (as well as in other branches of science, by the way) and dismissing proper foundation and justification of scientific propositions. More on these problems is to be found in section III.1 (also cf. note 72).

Since I am going to deal with the survival problem in section III.3, it may suffice to emphasize here that, of course, I agree with Mr. Hoebens that the available evidence for survival is "hopelessly weak" at best. And I am equally sure as he appears to be that my second recommendation is very likely to be ignored just by those psychical researchers who should rather take it to heart (several people whom I would classify as survival researchers have been invited to participate in this dialogue -- not even one of them revealed his thoughts). However, this does not dispense from the necessity, I think, to make this recommendation despite its possibly predetermined futility.

Since Mr. Hoebens probably knows better than anyone else what I think about the desirability of cooperation between proponents and critics of parapsychological research, I suspect that at least parts of his comments on my sixth recommendation were mainly intended to provide me with the opportunity to be somewhat more specific on what may appear to be a source of possible misconceptions in my original paper. I believe that "parapsychologists cannot afford to ignore" anyone's criticism. Therefore, my invitation to cooperate was, of course, meant to include any critic who believes that he has something to say on the problems at issue even when, as seems to be the case with Dr. Wimmer, this belief is based on a misconception. What I wrote about criticisms as they were put forward by Price, Hansel, Wheeler, and even Prokop was by no means intended to exclude them from the range of those people whose criticisms are worth our attention. I may add, however, that James Randi's recent disclosure of his "Project Alpha"<sup>22</sup> does not come up to the sort of thing I had in mind when I recommended closer cooperation between proponents and critics. It should be noted that I do not have serious objections against Randi's project itself but rather against the way he presented his "findings" or allowed them to be presented<sup>23</sup>. As far as I can judge, he has grossly exaggerated the representativeness of his "findings" which makes the whole affair rather counterproductive. A recent Dutch paper on the value of criticisms from opponents of parapsychology by Dick Bierman likewise, though with reversed premises, falls short of what I recommended in my paper<sup>24</sup>.



I did not write my seventh recommendation with the explicit intent to "infuriate the editorial staff of the neo-obscurantist magazine Esotera and Esotera's pet parapsychologist, Mr. Elmar Gruber." I have done that elsewhere<sup>25</sup>. But, of course, I would welcome it if they were concerned by this recommendation. There are some fundamental differences between "testing Cosmic Awareness" and "testing psi" as far as the testing situation is concerned. Since similar differences are also to be found between "testing psi" and "testing the survival hypothesis," I am going to discuss them in section III.3. It is with this proviso that I am ready to subscribe to Mr. Hoebens' amended version of my seventh recommendation.

I should perhaps conclude by stating that I had by no means intended snubbing people like Mr. Hendry with my final recommendation. I trust, however, that Mr. Hoebens will agree with me that there are quite a few people belonging to the occult lunatic fringe who can hardly be snubbed too frequently.

#### Response to Brian Inglis

It is quite obvious that Dr. Inglis and I strongly disagree with each other with regard to all of my seven recommendations. Since the extent of our disagreement is apparent even from a quick glance at our respective papers, I will restrict myself to only a few comments on what appears to be a diametrical opposition.

Ad 1: I completely fail to see how psi, "if it exists" (my underlinings), could "modify" or even "subvert" scientific method. This argument is clearly circular: To be in the position to justify his claim, i.e. to demonstrate that psi, if it exists, would modify or subvert scientific method, Dr. Inglis must first be able to prove the existence of psi by way of applying scientific method. But if he succeeds in doing this, the claim that psi, if it exists, modifies or subverts scientific method can no longer be justified since scientific method would already have been successfully used for proving the existence of psi. All that could possibly be assumed is that, even if psi exists, scientific method would be inappropriate to demonstrate this. But if that were true, the question whether parapsychology is possible as a science would already be negatively decided.

Ad 2: I may refer Dr. Inglis to section III. 3, below, where I will be trying to support my "surely absurd" argument.

Ad 3: I continue to enjoy what Dr. Inglis regards as my "behaviorist eccentricity," and I still hold that the phrase "anecdotal evidence" is contradictory in terms (see my response to Dr. Beloff).

Ad 4: Dr. Inglis' comment does not touch my recommendation, since each technical term is "a convenience," regardless of whether or not it is appropriate.

Ad 5: It is by no means clear whether decline effects form what Dr. Inglis calls "consistent inconsistency" (his underlining). I do not think this can be justly claimed in the present situation.

Ad 6: Finding flaws in parapsychological experiments necessarily requires the existence of such flaws. So, skepticism must not necessarily be corrosive<sup>26</sup>.

Ad 7: Obviously, Dr. Inglis misinterprets Martin Johnson's term "parapornography." And, again, his appeal to mesmeric trance does not touch my argument.

On Dr. Inglis' summing up: he demands that "scientists, let alone skeptics," should not be allowed to apply "procrustean research regulations." He is quite right, provided that we ourselves strive for strict research regulations and acceptably high methodical and methodological standards.

#### Response to Jürgen Keil

I largely agree with what Dr. Keil says in the first two paragraphs of his highly interesting comments. Like Dr. Beloff, he obviously fails to realize, however, that I was mainly concerned with his type (2) strategies and only indirectly with his type (1) strategies. Consequently, I would hold that no "particular decision" should be "made primarily because of (1)," while largely disregarding (2). That is to say that we should bother less about "unfavorable comments, ridicule and other negative reactions under (1)" than about the questions whether our scientific actions and propositions are justifiable under (2) and whether we are able to demonstrate this justifiability. Moreover, I did not say that "scientific research must have a concrete purpose" (my underlinings), but rather that any scientific action must have a concrete purpose since otherwise we will not have a criterion to distinguish between failures and successes of these actions<sup>27</sup>.

Now, Dr. Keil wonders on what sort of philosophy of science my recommendations might be based. Well, the conception closest to my own is that of the so-called constructive or methodical philosophy as advocated by the "Erlangen School" of philosophy of science. Evidently, it is impossible here to provide readers with an adequate outline of this philosophy. Suffice is to say here that it calls for a stepwise and full foundation and justification of any scientific action and proposition (at least, scientists should be able to give such justifications upon request), and that it holds that the task of philosophy of science is not only a descriptive, but also a normative or prescriptive one<sup>28</sup>. I agree with Dr. Keil that Occam's razor is certainly double-edged, and every shaver should be extremely careful when handling it.

Ad (H 1): Confer with the first paragraph of my response to Dr. Blackmore as well as section III. 1.

Ad (H 2): I largely agree, but confer with section III. 3. I would be interested to learn, by the way, why Dr. Keil believes, as he states in another of his papers<sup>29</sup>, that "recent research about near-death experiences" may have "probably weakened" his otherwise non-survivalist position.

Ad (H 3): To defend Hoebens here, I think that there is not much left of Bender's celebrated Pirmasens chair test after Hoebens' critical re-evaluation. Carrying out a similar investigation of an orthodox psychological claim, as Dr. Keil suggests, would at best lead to the weakening of a psychological case but could not serve for excusing shortcomings in parapsychological studies. Contrary to Dr. Keil's apparent opinion, I continue to view laboratory experiments as much superior to spontaneous cases as far as their evidential value is concerned, since with spontaneous cases the conditions of their occurrence are completely out of our hands. For a response to Dr. Keil's unfounded claim that "Hoebens only selected 'chair' tasks which had methodological weaknesses" (my underlinings), see Hoebens' "Letter to the Editor" in this issue of 25.

Ad (H 4): Again, in contrast to Dr. Keil's opinion, I do not think that we should first "advance to a point where detailed reliable findings reveal significant aspects of psi" before we put the linguistic means of the field in order. A proper terminology would already be desirable for describing and talking about those "detailed reliable findings."

Ad (H 6): It is true that exchanges with critics frequently become public relations exercises rather than useful discussions and that such exchanges usually take considerable portions of our time. But if at least parts of the critics' objections against parapsychological practice can be shown to be valid, then there may indeed be very good reasons for such time-consuming discussions. For the above reasons, I can not help having some doubts about Dr. Keil's doubts about some of my recommendations.

#### Response to Stanley Krippner

Needless to say that I was extremely pleased by the most flattering comments made by the current president of the Parapsychological Association. I find myself in full agreement with Dr. Krippner's remarks on my recommendations 1, 4, 5, 6, and 7. To me, especially his comments on my sixth recommendation seem to be of considerable importance. I wholeheartedly support his suggestions that more skeptics and critics write for parapsychological periodicals and that they join the Parapsychological Association (also cf. the first part of my response to Dr. Stokes).

Ad 2: I do not believe that, as far as the results are concerned, science might reach anything by way of speculation that could not equally well (and probably better) be attained by other, non-speculative means. In my view, the only useful function speculation can have in science is that of stimulating new research. I share Dr. Krippner's assessment of the recent extensive paper on survival research by William G. Roll (at least as far as its first six chapters are concerned)<sup>30</sup>, while I have some reservations with regard to Rupert Sheldrake's concept of "morphogenetic fields." Since I was not yet able to read Sheldrake's book, however, this assessment is only based on two relatively brief papers on his concept published in the Parapsychological Journal of South Africa<sup>31</sup>. Therefore, I will reserve further judgement for the time being.

Since I regard my seven recommendations as necessary but by no means as sufficient guidelines for the future practice of parapsychology, I am especially pleased that Dr. Krippner adds "three more recommendations for parapsychology's future." I find these additional recommendations really excellent, and I will seize the opportunity to briefly comment on each of them.

Ad 8: This recommendation is already to be found in an earlier publication by Dr. Krippner<sup>32</sup>. Perhaps editors of parapsychological journals should consider joining forces to elaborate guidelines as to how to deal with experimental protocols which cannot be published in full for whatever reasons, but knowledge of which may nevertheless be indispensable for reaching adequate conclusions on the appropriateness of the applied experimental procedures, on precautions taken against fraudulent manipulations, etc.

Ad 9: While I do not agree to all of Ramakrishna Rao's theoretical considerations on the problem of replication (see my papers referred to in note 15), his recent proposal<sup>33</sup> to form a group of "psi-conductive" researchers ("successful psi elicitors," as he calls them) may help solve some of the problems at issue.

Ad 10: Also agreed; perhaps some sort of inter-laboratory arrangements could be envisaged to coordinate long-range programs.

Finally, I am immodest enough, I fear, to hope that Dr. Krippner may be right with the flattering comments he makes in the concluding paragraph of his manuscript.

#### Response to Morton Leeds

Ad 1: I agree with Professor Leeds on his first comment, except for the fact that he seems to be presupposing that there are "revolutionary implications of the field's endeavors." Moreover, I fear that, in the long run, revolutionary outlook may well lead to invalid scientific methodology and serve, say, to excuse methodological shortcomings.

Ad 2: The point I wanted to make was not that survival research should be given up because of our "current ignorance," but because I think that, on principle, survival cannot be investigated scientifically. See section III.3.

Ad 3: I did neither dispute that, traditionally, spontaneous cases have provided "the main drive" for parapsychological research nor that such cases provide "most interesting material." I merely turned against ascribing any evidential value to them. We can never know for sure whether a spontaneous occurrence was, in fact, due to a paranormal factor. And accumulating such weak and inconclusive cases (as far as their evidential value is concerned) will never result in a strong and conclusive one.

Ad 4: If a terminology "is becoming very uniform," this does not imply that, at the same time, it is also becoming very adequate. While uniformity is a necessary criterion of a scientific terminology, it is by no means a sufficient one. It is true that the field is totally under-funded; but for constructing a proper terminology only little more than paper and pencil is required.

Ad 6: Hövelmann has not chosen to ignore the literature! What is needed is rational and constructive cooperation rather than merely answering criticisms. Professor Leeds' 80 percent figure could only be justified if any parapsychological experiment is also regarded as "answering the critics." I would not recommend adopting this manner of speaking.

Ad 7: Even a "dog" is capable of warding off "flies." Thus, the "parapsychological dog" should make plain to the "pseudoscientific flies" that they will not be allowed to intrude into his field. In fact, the problem in question is a more serious one than Professor Leeds seems to realize. The results of Allison's<sup>34</sup> survey of the views of the members of the Parapsychological Association, for instance, suggest "that laymen do, in fact, have a 'corrupting' influence on the field"<sup>35</sup>. Allison concludes:

"Additional analyses show that those who interact frequently with laymen are less likely to condemn 'fads and borderline areas,' more likely to support research on post-mortem survival, and less likely to say that the occult movement will be harmful to parapsychology"<sup>36</sup>.

To conclude, I have no objections against "the continued slow, steady growth of technique and knowledge" provided that it can be secured that this growth is well-controlled and not exuberant or running wild.

#### Response to Walter von Lucadou

Dr. von Lucadou is one of the two commentators (the other being Dr. Pinch) who generally questions the usefulness of any enterprise making recommendations for the practice of a special branch of science. I would have a lot of things to say in reply (and I have done so in personal correspondence with Dr. von Lucadou); but, unfortunately, I am forced to restrict myself here to dealing with with one aspect only for reasons of space. He claims that the history of science has taught us that recommendations put forward by a philosopher of science are usually ignored. I do not agree with this for the simple reason that the prevailing "analytical" philosophy of science (including Logical Empiricism of the Vienna Circle, Sneed's Structuralism, Critical Rationalism of the Popperian school of philosophy of science, historicism of the Kuhnian and Feyerabendian brand, as well as various other positions derived from these philosophies) have never, with normative or prescriptive intent, put forward any recommendations as to how members of a special branch of science should organize their practice, build their theories, construct the linguistic means of their field, etc. Quite to the contrary, these philosophies have always regarded factually existing scientific practices as being the best which are possible at any given time, and they have even explicitly and programmatically excluded all normative efforts from their scope (so, for instance, Carnap and Stegmüller). It can easily be seen that this can at best lead to an affirmation of the analyzed factually occurring scientific practices. So, I do not think that history of science has ever demonstrated the futility of attempts to suggest norms and prescriptions for scientific practises. Nevertheless, Dr. von Lucadou may turn out to be right that my recommendations are of no use. But we cannot know this before such an attempt is made.

Ad 1: I largely agree with Dr. von Lucadou. However, I would take issue with his opinion that quantum theory was a revolution in physics, since the truth-conditions of propositions made in quantum physics remain largely the same as in classical physics<sup>37</sup>.

Ad 2: See section III. 3.

Ad 4: I agree with Dr. von Lucadou's remarks. However, they do not place any restrictions on my recommendation since the formulation of such "phenomenological models" should already make use of an appropriate terminology.

Ad 5: I appreciate Dr. von Lucadou's criticism of my inadequate use of the term "inconsistency." Of course, he is right saying that the term "inconsistency" is a "predicator" (cf. section III. 2) denoting properties of models or theories and, hence, cannot be ascribed to constitutive elements of paranormal (or other) events. I avoid this term in the present paper and occasionally speak of "contradictory experimental findings," instead. I would take issue with Dr. von Lucadou, however, when (if I understand correctly

what he means) he holds that "underlying models" which "are not formulated explicitly" (my underlinings) may eventually lead to inconsistencies when confronted with empirical findings. At one point at least, these underlying models must be explicitly formulated since otherwise it would be impossible to find such inconsistencies by comparing the linguistic representations of these models to the linguistic representations of the experimental findings. Such linguistic representations are indispensable since one can not compare a distorted spoon to a model.

Ad 7: I agree that the unpleasant disputes about the acceptability and justifiability of parapsychological research will not be settled by an experimentum crucis. I further agree that we should be "modest" and "patient" as far as parapsychology's official recognition as a legitimate branch of science is concerned. I would oppose, however, if this demand were intended to be applied also to the rigidity of our methods.

#### Response to Gerald C. Mertens

I much appreciate Professor Mertens' attempts to "heap praise (and all other kinds of positive reinforcers)" on me.

Although I find a number of behavioristic claims quite reasonable, I think that there can well be pointed out methodical restrictions<sup>38</sup> on the validity of behavioristic propositions (and all the more so if they are made from a position that tries to out-Skinner Skinner). In my response to Professor Zusne's comments, I will have something to say on where I stand on Professor Mertens' expanded version of Professor Truzzi's "hard line continuum." So I may defer this discussion for the moment.

Of course, I am extremely pleased by the rather favorable comments by a critic writing from the extreme hard line position (not many "parapsychologists"<sup>39</sup> have been lauded that way), and I fully agree with him that many of my recommendations may equally apply to psychology, sociology, or psychiatry (this being an aspect that was not realized by Professor Zusne although he deals with this question in his comment). As I have already mentioned in my response to Dr. Krippner, I do not regard my series of recommendations as a complete one. So, I welcome and wholeheartedly agree with the additional recommendation Professor Mertens says he missed most in my paper, i.e. that the data are presented in such a way as "to insure the study is free of frauds, dupes, getting excited over 'chance,' exaggerations, or the 'real world principles' <sup>40</sup> ... at work in the situation which go unnoticed and/or unreported." Admittedly, taking adequate precautions against fraudulent manipulations is extremely difficult with such mediums as Uri Geller and others, but this objection does apply to much less a degree (if at all), for instance, to Schmidt-type experiments. Be that as it may, I hope that my recommendations have made a contribution to the effect that, in future some dubious "E.S.P.'ers" will have fewer reasons for "laughing all the way to the bank with" Professor Mertens' "money, time, and effort."

Now, Professor Mertens reproaches parapsychology for being at least indirectly responsible for "creeping 'irrationality' toward the likes of a Holocaust or Jonestown." This fatally reminds me of what I recently read in a German police journal<sup>41</sup> or of arguments as those to be found in the libels authored by the German critic of parapsychology, Wolf Wimmer. In a similar way to Professor Mertens's, the anonymous author of the police journal article relates parapsychology to the Jonestown mass suicides and to outbreaks of terrorism, while Wimmer holds that parapsychologists must share responsibility, in retrospect (!), for medieval witch hunts<sup>42</sup>. It is true that a few parapsychologists have occasionally shown a close affinity to obscure mystical, supernatural, or other basically irrational movements; but to claim that, because of this unpleasant fact, parapsychologists (in plural!) are to be held responsible for terroristic activities or other criminal outrages is absurd to such a degree (all the more so if such claims are made without any further specifications) that it simply does not merit detailed refutation. I would agree with Marcello Truzzi who recently wrote:

"Superficial correlations such as that of nazism with some German occult views should not be translated as causation; and this view overlooks occultisms elsewhere such as the occult boom in England during the same period"<sup>43</sup> (my underlining).

Professor Mertens' "catch 22-like problem on the attention variable" as viewed through a behavioristic monocle, likewise does not hold water. I do not believe that he would find an example in the parapsychological literature showing that sound criticism, say, of inadequate experimental conditions, has resulted in intentional reiterations of these inadequacies caused by "reinforcement by attention" rather than in (at least more or less willing) attempts to improve these conditions. Moreover -- and this shortcoming appears to be at the roots of Professor Mertens' arguments in this part of his comments -- he continuously confounds experimental parapsychology with the performances of dubious (though well-dressed) psychics. Thus, my first personal recommendation for "the world's worst dresser" should perhaps read: In order to assess the scientific acceptability of parapsychological research, decide to base your judgement on reports of well-controlled laboratory experiments by parapsychologists rather than on the dubious performances and extravagant dressing habits of a questionable "prime psychic of our days"! Critics should always seek to make their case against the strongest arguments of parapsychologists.

To turn back to the core of the additional recommendation Professor Mertens briefly outlines, I certainly agree with him that all information relevant to the fraud hypothesis should be made available. I may perhaps point out, however, that there is a considerable number of papers in the parapsychological literature dealing explicitly with the question of how to take adequate precautions against fraud. That the problem of such adequate precautions against fraudulent manipulations is a well-recognized and constantly considered one among parapsychologists, may also be inferred from the following: The study program of the Parapsychology Department at John F. Kennedy University includes two courses relevant to the problem of fraud. In an Undergraduate Course, entitled "Parapsychology, Magic, and the Skeptics," various "ways in which conjuring tricks can be and have been used by fraudulent mediums and others"<sup>44</sup> are explained and discussed.

And in a Required Graduate Course, entitled "The Creation of Illusions," various "principles and techniques of the professional illusionist's trade are explored in order to develop awareness of the possibility of fraud in psychic claims"<sup>45</sup>. Likewise, two magicians, Loyd Auerbach and Bill Daniels, on October 16, 1982, conducted an all-day workshop on magic and mentalism at the American Society for Psychical Research. Reports Auerbach:

"Some of the stage-magic principles ... covered were basic sleight-of-hand, misdirection, various types of 'forces' (forcing a word or object on a person, who would mistakenly think the whole thing was free choice), two-person telepathy codes, blindfold techniques, and basic methods of simulating clairvoyance, psychokinesis and precognition"<sup>46</sup>

Auerbach especially emphasizes the need "for laboratory conditions where the experimenter, not the subject, has control"<sup>47</sup>. This may suffice here to demonstrate that the problem of fraud is not at all disregarded in parapsychology although, of course, there are still many things requiring improvement. Moreover, the 1983 Convention of the Parapsychological Association in Madison, N.J., featured a session (organized by Marcello Truzzi) that invited conjurors to discuss all aspects of involvement of conjurors in psychical research.<sup>48</sup> I trust, moreover, that Professor Mertens has realized in the mean time that his own recommendation is essentially equivalent to the first of the additional recommendations which Dr. Krippner made in his comments on my paper. May I remind Professor Mertens that Dr. Krippner currently serves as president of the Parapsychological Association?

Contrary to Professor Mertens' opinion, I do think that to specify what fraud precautions were taken, in fact, does mean "excessive requirements of researchers in this area compared to other areas of inquiry" (my underlinings). But I believe that these additional requirements are well justified given the very nature of parapsychological claims.

Reading the other commentaries on my recommendations, Professor Mertens will possibly notice that my position is perhaps not quite as "lonely" as he appears to assume, although many commentators are very critical of my views (nevertheless, I appreciate Professor Mertens' pity). I especially welcome that he has at least decided to "keep watching good research methodology" from "out there." Nevertheless, my second personal recommendation for Professor Mertens' (and parapsychology's) possible benefit should read: Move a few steps closer to parapsychology, and your range of vision will improve considerably!

#### Response to Robert L. Morris

Ad 1: I essentially agree with Dr. Morris' remarks, except for his argument that orthodox methods may eventually lead to revolutionary findings (see section III.1.).

Ad 2: I would have no objections against the sort of research Dr. Morris outlines under point (1). I think that these problems can, in principle, be profoundly investigated and may eventually lead to useful

insights into the factors that contribute to anomalous human experiences. Those investigations can not be regarded as tests of the survival hypothesis, however. The usefulness of point (2) largely depends on the outcome of research under (1).

On Dr. Morris' "minor point": I am well able to clear up this apparent contradiction. The phrase "aging Nobel laureates" was meant to point to the fact that a considerable number of well-reputed scientists (Werner Heisenberg, Pascual Jordan, Hermann Oberth, and many others may serve as examples) obviously feel entitled to add a lot of ill-founded ideological or metaphysical trash to their earlier solid work. Now, these people must have reached a certain degree of fame and reputation rather than a certain age to be sure that their speculations will be taken seriously. So, Werner Heisenberg, who was appointed Nobel laureate when he was 31 years old, meets the above description with regard to his age as well as with regard to his ideological speculations. I admit that this is not a very favorable view of some of our scientific celebrities, but I think it can be justified by analyzing their ideological, cosmological, etc. writings.

Ad 6: Dr. Morris' remarks on my sixth recommendation are excellent. I have no objections against his broadened version of this recommendation, and I am glad to see that he supports my efforts to break the unpleasant parapsychologist/critic dichotomy in such an unmistakable way (also confer with the first part of my response to Dr. Stokes). We should not completely lose sight of the fact, however, that "cooperative" as well as "competitive" strategies are already to be found within parapsychology<sup>48</sup>.

Ad 7: Of course, I would regard studies as those outlined by Dr. Morris as entirely scientific. I did not plead against studies of those types; I merely recommended (a) that we do not accept ill-founded pseudoscientific concepts (whoever may submit them), (b) that we not let irresponsible groups or individuals intrude into the field, and (c) that we explicitly point out (especially to the critics) that and why those groups or individuals are irresponsible. To be able to do (c), research of the kind suggested by Dr. Morris (or that conducted by Professor Truzzi in San Francisco) may even be a necessary precondition.

To comment on Dr. Morris' concluding paragraph, I was in the first place concerned with being a good scientist by rational and reasonable standards rather than with being "a good scientist by today's standards" (my underlining).

#### Response to Carroll B. Nash

In my responses to other commentators, I have already answered many of the points Dr. Nash raises (or I will do so in section III, respectively). Therefore, I will confine myself to making only few comments here.

Ad 2: I do not think that the survival hypothesis and the super-ESP hypothesis are the only hypotheses worth considering in view of the "absurd" (D.M. Stokes) data of survival research. There may probably be several non-survivalist as well as non-parapsychological hypotheses to account for

considerable parts of the data. They should not be dismissed prematurely.

Ad 3: I am not convinced that the occurrence of psi has yet been established beyond reasonable scientific doubt. Dr. Beloff obviously shared my opinion when, in his presidential address at the Cambridge P.A. Convention, he said:

"In calling them [the questions of the existence of psi, the nature of psi, and the survival question] 'open questions' I wish to imply that, in present circumstances, a perfectly rational case can be made for answering them in the one way as in the other" (my underlinings)<sup>49</sup>

Therefore, I would take issue with some of Dr. Nash's arguments in the rest of his comment on my third recommendation.

Ad 4: "New concepts" should themselves make use of properly defined terms (also cf. section III. 2). By the way, I suggest that some of the most agile inventors of new concepts occasionally take a breathing-space. A new concept every other week does hardly contribute to the consolidation of the field).

#### Response to Irmgard Oepen

I thank Frau Professor Oepen for her comments on my views (and Mr. Hoebens for being so kind as to compile them). There has been a protracted dispute between Frau Professor Oepen and myself during parts of the years 1981 and 1982 caused by my lengthy review<sup>50</sup> of a medical dissertation<sup>51</sup> that was written under her supervision. The informal conference she mentions, held in Marburg in November 1982, has certainly made plain to both of us that parts of our differences were probably due to premature and incorrect assessments of the respective positions we take in matters parapsychological. Frau Professor Oepen mentions that our conference resulted in an agreement to join forces in publicly exposing quackery. Moreover, there turned out to be nine basic points to which all of the five participants of the conference found they could agree. I very much hope that such informal talks and the agreed-upon points might serve as a model for other dialogues between proponents and critics of parapsychological research.<sup>52</sup>

To turn to Frau Professor Oepen's comments, she is quite right (and seems to support my view) with what she writes about necessary and sufficient conditions for scientific respectability. If Frau Professor Oepen bothers to read the other comments on my paper, she will probably realize that I am by no means the only one who "does not fit the stereotype of the credulous psychical researcher" (I may especially call the comments by the parapsychologists Dr. Beloff, Dr. Blackmore, Dr. Krippner, Dr. von Lucadou, Dr. Morris, Dr. Palmer, and Dr. Stokes to her attention).

I agree with her, moreover, that so far there is little (if anything) in parapsychology that could justly be called "substantial findings." I believe, however, that there are enough "anomalous" results which merit closer inspection. I should point out that, since parapsychology has never been adequately funded since its inception a century ago, Frau

Professor Oepen's opinion that "financial encouragement of parapsychological research projects is simply a bad investment," cannot be an empirically founded one (even if it were true).

I completely share her concern (and especially my seventh recommendation of parapsychological concepts and findings for impure purposes, particularly in so-called "alternative medicine," and I am certainly willing to support her struggle against medical humbuggery and charlatantry.

To conclude, I interpret Frau Professor Oepen's comments on my paper as a welcome confirmation of our recently established improved communication. And I am especially pleased to see that she takes a decidedly non-Wimmerian position.

#### Response to John Palmer

Dr. Palmer has exactly understood what I intended to point out with my recommendations 1 and 4, and I am glad to see that a distinguished researcher of his calibre fully shares my views. He has clearly realized what is probably the most important terminological problem confronting contemporary parapsychology: the difference between explaining and identifying an anomaly.<sup>53</sup> Our problem is largely caused by the fact that, as Dr. Palmer convincingly points out, "we use the same set of terms to label what we seek to explain ... as we use to label the principle or process which ... might serve to explain it." (For a further discussion of the problem of scientific terminologies and some suggestions as to how it might be solveable, cf. section III. 2).

As regards my recommendations 2, 3, and 5, I think the main difference in our opinions lies in that Dr. Palmer holds that "evidentiality is a matter of degree," while I think that there must at least be some pre-terminated and operationally well-defined criteria for the assessment of evidentiality, that is, for what we are ready to accept as evidence, since otherwise science would be unsuitable to produce knowledge. (Contrary to Dr. Palmer's opinion that the decision as to what should be accepted as evidence is necessarily "to some extent subjective," I would hold that this decision can be reached inter- or transsubjectively in a rational discourse<sup>54</sup> about the purposes scientists are planning to pursue in their research). This difference in our opinions had implications for all of these three recommendations (2, 3 and 5). Dr. Palmer believes that some of my arguments would also apply to parts of normal psychological research. As I stated in my response to Professor Mertens, I really think they do. I did not claim, however, that pieces of research (to turn back to our differences) should be treated as either conclusive or "worthless." Doubtlessly, investigation of spontaneous cases can be of importance by producing new ideas for strict experimental testing and in many other ways. However, as I have repeatedly emphasized in my responses, we should refrain from ascribing any evidential value to them.

Dr. Palmer holds that "it is both legitimate and understandable that we want to know something about our own nature and destiny," even if science should not be able to provide "CONCLUSIVE answers." I find this understandable, too. Since I think, however, that it can be demonstrated that science, on principle, is incapable of providing even tentative answers to such problems, the question immediately arises: What do we really know about our nature and

destiny if the answers we are able to obtain are necessarily inclusive (relative to the above-mentioned criteria for the assessment of evidentiality)? In this case, I fear, we know NOTHING at all! I share Dr. Palmer's conviction that "some information is better than no information." But we can only reasonably claim to dispose of "some information" if we are already able to decide what to accept as relevant information on the problems in question. And for that, we must already be able to distinguish conclusive from inconclusive answers. Otherwise we would not be in the position to know whether what we have is "some information" or whether it is no information. (To avoid possible confusion, I should mention that the term "information" has not been used in the sense of (any) information theory, neither by Dr. Palmer nor by myself).

#### Response to Trevor J. Pinch

Like Dr. Beloff and some other commentators, Dr. Pinch seems to believe that I was mainly concerned with public-relation matters, whereas I rather intended to recommend that we speak and act in a rational way and to point that official recognition will only come as a by-product of this. In contrast to Dr. Pinch (and to his probable surprise), I do think that physicists likewise need advice with regard to the truth-conditions of the propositions they produce, with regard to their ways of speaking about experiments, nature, etc., and especially with regard to an adequate series of steps to introduce the fundamental concepts of geometry, chronometry, and hylometry. Geometry, chronometry, and hylometry are a-priori theories which make empirical measurement of space, time and material "possible." They have to be established before (in a methodical sense) physics in the modern sense of an empirical science can begin. Therefore, these three disciplines belong to protophysics. True sentences of protophysics are those sentences which can be defended on the basis of logic, arithmetic and analysis, definitions, and especially ideal norms which make measurement possible. These sentences can (and need) neither be founded empirically nor falsified empirically, since empirical physics is based on them. Thus, "non-normality of parapsychology" cannot be inferred as Dr. Pinch does, from the mere fact that the field obviously needs advice.

Dr. Pinch then proceeds to ask whether the strategies I recommend "embody a realistic picture of the practice of orthodox science and scientific change." If readers compare my section III. 1, below, with Dr. Pinch's publications,<sup>55</sup> they can easily see that our respective views of what is "a realistic picture of the practice of orthodox science and scientific change" are diametrically opposed. Therefore, it does not come as a surprise that he largely disagrees with the opinions I have advanced in my paper, and especially with my arguments against revolutionary pretensions.

Ad 2: I would be most interested to learn why "it is unwise for sociologists and philosophers to make recommendations"? (my underlining). Since merely analyzing factually existing scientific practices can at best lead to affirmative statements about these practices, I believe that philosophers of science have the obligation to make recommendations for the improvement of such practices. This is the only *raison d'être* philosophy of science can justly claim. Unfortunately, most philosophers of science have chosen to resign themselves to being mere chroniclers of science.

Ad 3: It is inappropriate to compare the question of the existence of psi to that of the existence of meteorites, since in most spontaneous (and many experimental) cases in parapsychology only verbal reports are available, while meteorites remain in our hands as material objects. That is, a comparison of the type which Dr. Pinch proposes does not take into account that different methodical levels are involved. Moreover, we are able to construct idealized physical models (in the strict sense of "model" as they are used, say, by architects) of meteorite falls to test our respective hypotheses about the "behavior" of meteorites. An adequate procedure on the same methodical level could perhaps be to compare the existence of meteorites to that of a "permanent paranormal object" in John Beloff's sense<sup>56</sup> (i.e. of an object the paranormality of which is self-evident). And even this procedure can be questioned.

Ad 4 and 5: Confer with my responses to Dr. Beloff and Dr. von Lucadou, respectively.

Ad 6: I find Dr. Pinch's remarks on this recommendation extremely counterproductive, especially since I believe that many parapsychologists and many critics have essentially the same interests (also cf. my responses to Dr. Keil, Dr. Stokes, and Professor Zusne).

#### Response to Steven M. Rosen

I regret to say that I am especially unsatisfied with Dr. Rosen's comments on my paper. I was a bit amused (but also upset, at the same time) to see that he takes great pains to unmask me as an adherent of "objective" or "naive realism." Just me! I completely fail to see what was in my paper that led Dr. Rosen astray. I may here place on record, once and for all, that I hardly know of any other philosophical position that I find less acceptable than "objective realism," which I criticize wherever and whenever the opportunity arises. Since it can easily be demonstrated that any form (and even any attempt to argue in favor) of objective or naive realism must necessarily lead to logical inconsistencies (not to mention other very serious shortcomings), I have always most sharply criticized objective realism in my parapsychological writings<sup>57</sup> and especially in my writings in the philosophy of science.<sup>58</sup> Upon rereading my paper, I cannot find any passage that could be construed as dealing with what is "objectively out there," as Dr. Rosen puts it. Furthermore, I have claimed nowhere that science was a "fact-gathering" activity (which would be sheer nonsense, in my view); I would rather hold that science (at least the natural sciences and the majority of the other branches) is a fact-producing activity. To be able to reject objective realism, it is completely superfluous to make pretentious appeals to quantum physics or to borrow arguments from the works of Bohm and Capra (I find their work methodologically naive and short-sighted, by the way; there is no reason for throwing rationality and reason overboard together with objective realism or for replacing objective realism with an "aesthetic way of doing science"<sup>59</sup>). A critical analysis of the arguments of proponents of critical realism and of their linguistic means is already quite sufficient to demonstrate that this philosophical position is untenable.

I also disagree with Dr. Rosen when he holds that -- at least "by all indications" -- "the subject-matter of parapsychology is revolutionary"

(his underlining). I simply fail to understand this sentence. How could a "subject-matter" be revolutionary? Even if one believes that the use of that term should not be given up, it could only make sense to label a human action "revolutionary." And whether a particular action is justly characterized as being "revolutionary" can only be judged relative to a theory of action that contains normative elements which provide the means to distinguish revolutionary actions from non-revolutionary ones.

In short, I cannot help noting that Dr. Rosen and I appear to disagree with each other in the strongest possible way. The only point of agreement seems to be our rejection of "objective realism," and even this rejection is obviously based on very different reasons.

#### Response to Gertrude R. Schmeidler

I was glad to learn that Dr. Schmeidler agrees with my last four recommendations, and that these were so clear to her that she hopes it was "unnecessary ... to state them." If she bothers to take a look at the other commentaries, she will probably notice, however, that these recommendations seemed not so clear to anyone. Therefore, I continue to believe that it is was necessary to state them. Moreover, close inspection of the literature reveals that it was all but unnecessary to make these recommendations. Now, Dr. Schmeidler fears that I may be out to "argue against exploring unmapped areas." That is certainly not the case. I merely argue that we should not embark on unscientific approaches to such "unmapped areas."

As regards my first recommendation, I would not mind, of course, if parapsychologists or other scientists "hope that their next experiments will be ... insightful, deeply important, provocative." However, I have two things to say here. First, what I intended to caution against was that such hopes (or claims) can (intentionally or unintentionally) lead to a worsening of indispensable methodical rigidity and methodological standards. And second, I do not think that such hopes, even if important changes are envisaged, should be called a "revolutionary outlook."

The questions which arise from Dr. Schmeidler's comment on my second recommendation are: 1. What do we learn about survival from "objective description of ... ambiguities"? 2. What does it get us if authors state their "personal opinions" (my underlinings) on the "weight of the evidence"? The answer in both cases, I fear, must be: nothing. I do not see how "personal opinions" could be anything other than personal opinions. This does not question the fact that personal opinions can well be useful for reaching interpersonal knowledge.

I have no quarrel with Dr. Schmeidler if her request to "rely heavily" on spontaneous cases is only to say that "they can lead to the hunches from which brilliant new research may emerge." In my paper, I did not object against this way of utilizing spontaneous case material. From her remark, however, that we should be "willing to take the risk," I infer that she hopes for more from the examination of such material.

As Dr. Schmeidler requested, I have tried to "modify" the formulations (though not the contents) of two out of my first three recommendations in such a way as to meet parts of her objections. But I am really not sure whether this will completely satisfy her. It should have become clear, that I do not want to restrict "freedom" of inquiry. I maintain, however, that scientists should always be able to argue rationally for what they are doing

under the protection of freedom of inquiry.

#### Response to Christopher Scott

I think Dr. Scott has misconceived the purpose of my paper and that he underestimates the importance of language (or "talk") and presentational aspects in any science. I have several things to say in reply:

1. The main purpose of my paper was certainly not to call for a "better window display." Since I have already replied to this point in my response to Dr. Beloff's comments, I may refer Dr. Scott (and other readers) to this part of the present paper.

2. I am sure that Dr. Scott knows quite well that the quality of experiments in parapsychology differs widely. Therefore, I find it totally inappropriate to treat "the parapsychological experiments" all alike or to dismiss them all, in passing, as "bad experiments." Even in such a controversial field as parapsychology, it is not as easy to get away with bad experimental designs or sloppy evaluations as Dr. Scott pretends. Dr. Scott and I sat next to one another when Heinz Berendt presented his embarrassingly silly metal-bending film at the Cambridge Convention of the Parapsychological Association, and he must have realized quite well how this presentation was received by the vast majority of those parapsychologists present (I may especially remind him of John Palmer's sharp and unmistakable reaction).

3. Dr. Scott completely underestimates the role language and presentational aspects play in any science. Any criticism of a special branch of science must necessarily include a criticism of those presentational aspects and of that field's linguistic means. Presentational questions should by no means be considered a cura posterior. Kuno Lorenz has convincingly demonstrated that the set-up of a science cuts straight through the separating line of research and presentation. A rational and reasonable approach to science has always to consider with regard to the activities of scientists both research and presentation as well as their interdependence. Scientific research and scientific presentation ("talk," as Dr. Scott calls it) are two inseparable aspects of science. You cannot talk about one, while disregarding the other. Therefore, my recommendations were not attempts at improving tactics, but requests to standardize both means of research and the linguistic means of the field (also confer with section III. 2). For reasons of space, I am not able to deal further with the relations of research and presentation here. But I should strongly urge anyone (and especially Dr. Scott) to have a close look at Lorenz' papers I referred to in note 28 of this paper.

#### Response to Douglas M. Stokes

I thank Dr. Stokes for his detailed and intelligent remarks on my paper. I think that right at the beginning of his comments, he raises a very important point: that of the unpleasant parapsychologist/critic dichotomy and its consequences; I will seize the opportunity to explain my own thoughts on this matter which are very close to Dr. Stokes' apparent opinion. Interestingly, he ascribes to me the metaphysical predilections of the Randi group. Now, it is true that I am extremely critical of many trends in contemporary psi research and that I think that James Randi has

scored some good points over the years. On the other hand, I am also very critical of many things Randi does and especially of things he says (or refuses to say, for that matter)<sup>60</sup>. This may suffice here to characterize my own position; details are to be found in my response to Professor Zusne's comments, below. What I want to state here is that I fully agree with Dr. Stokes' opinion that anyone who, on the basis of the data of parapsychological research, argues in favor or against the existence of psi phenomena, is essentially acting as a parapsychologist, no matter whether he is a member of the Parapsychological Association or of the Committee for the Scientific Investigation of Claims of the Paranormal. Both the (ideal) "parapsychologist" and the (ideal) "critic" are essentially doing the same things: they refer to the same data, use the same logic of argumentation, try to establish which sort of conclusions are warranted with regard to a given data base, etc. What differs are the standards they apply for deciding on acceptance/rejection of certain claims and, dependent on these differing standards, the conclusions they draw. The actions they perform are essentially the same. Moreover, I fully agree with Dr. Stokes that the parapsychologist/critic dichotomy, still upheld especially by extremists on both sides, is completely insensitive as to the various "shades of gray," with regard, say, to the question of the existence of ESP. Why not mention names? As I see it, positions held by such "parapsychologists" as Martin Johnson, John Beloff, Douglas Stokes, John Palmer<sup>61</sup>, or Sue Blackmore are much closer to those held by their nominal opponents Ray Hyman, Marcello Truzzi, Piet Hein Hoebens, or Leonard Zusne than to those held by their "fellow parapsychologists" Ed Cox, Guy Playfair or Brian Inglis.

The term "Parapsychologie" was coined and introduced in 1889 by the German psychologist and philosopher, Max Dessoir<sup>62</sup>. He wrote:

"If -- analogous to such terms as paragenesis, paragogue, paracope, paraculus, paralogism, parergon, etc. -- para is to denote something that goes beyond or stands aside from the ordinary, then one could perhaps call those occurrences which stand out against the normal course of inner life, parapsychic, and the branch of science that deals with them, 'parapsychology.' A similar compound word, metapsychology, may serve as a precedent. This word is not a good one, but in my opinion it has the distinction of concisely characterizing a hitherto unknown border area between the average and abnormal, pathological states. And such neologisms do not claim more than the restricted value of practical usefulness" (Dessoir's underlinings)<sup>63</sup>.

I do not agree with everything Dessoir says here, but given this (or a similar) introduction of the term "parapsychology," James Randi may well qualify as a parapsychologist. In the light of all this, readers (and "critics" in particular) should perhaps reconsider Dr. Krippner's comments on my sixth recommendation.

To turn to Dr. Stokes' objections against my recommendations, I will answer them consecutively.

Ad 1: (a) Of course, I think that it is possible to form a reasonable assessment of the "competing schools in the philosophy of science" and to decide which should be accepted. But even without this, it is possible to



compile a catalogue of minimal requirements (if we discount Kuhn and Feysabend for the moment) for a rational scientific practice. (b) Dr. Stokes is right claiming that it is unprovable whether or not a researcher in the field is aware of what the request to be scientific "really means" (this would indeed require an "inspection of the mind" of that person). But it is well proveable if a certain researcher does not care about the request to be scientific (and to draw the rather favorable conclusion that he is unaware of this request), whether or not he is actually aware of what that means. (c) I agree that parapsychologists do reflect much more upon scientific methodology than do researchers in other fields (and since I am frequently engaged in discussions on philosophical and methodological question with physicists, biologists, chemists, linguists, psychologists, and members of some other scientific branches, I know quite well what I am talking about). But it is also true -- and I am inclined to believe that Dr. Stokes will agree with me on this point -- that in parapsychology scientific rules are still more frequently violated than in orthodox fields.

Ad 2: Dr. Stokes already presupposes that the "psi process" cannot be explained by "normal" means. But that is the very question at issue. I generally object to the use of the term "revolution" in science (see section III.1). But even if it is used, there is no basis for ante hoc claims of being "revolutionary," that is, no claim should be made that our work is "revolutionary" as long as it is still undecided whether it is "revolutionary." Moreover, I did not advocate abandoning scientific methodology in the first part of my paper, as Dr. Stokes apparently believes.

Ad 3: There is an apparent misconception on Dr. Stokes' part. What I wrote about the principal impossibility of strict repeatability in parapsychology does not contradict what he writes about the necessity of having at least some degree of repeatability. Of course a certain degree of repeatability is an indispensable requirement for any effect to be investigated scientifically. I do not have the well-established theory of psi phenomena Dr. Stokes is asking for. What I do have, however is a well-established theory of what an experiment is in the natural sciences as well as in other sciences and in parapsychology.<sup>64</sup> And I also have a well-established theory of what scientists in these various areas of research can maximally attain by their respective ways of experimentation; and strict repeatability is certainly unattainable for parapsychologists.

Ad 4: Dr. Stokes tries to justify survival research arguing that, in the long run, it might potentially provide an antidote against the prevailing "religion of materialism." I do not find this convincing. If you open a door between two pitch-dark rooms, it is unwise to expect that it will be getting light in one of them! (Also confer with the concluding part of my response to Dr. Beloff).

Ad 5: It is a well cared-for and much-beloved myth that such questions as those mentioned by Dr. Stokes were the central concern of science, I fear. They are unanswerable in principle. Since the underlying methodological problems are partly similar to those underlying the question of survival, I will briefly deal with them in section III. 3.

Ad 6: Confer with section III. 2.

In conclusion, then, I should again emphasize that despite my objections, I have much enjoyed Dr. Stokes thoughtful and reasonable comments, and I find myself in agreement with many of his opinions.

#### Response to Ulrich Timm

I have problems in accepting the imputation in the first paragraph of Dr. Timm's comments, that parapsychologists "occasionally disregard" the rules of science "out of forgetfulness."<sup>64A</sup> Since, by definition "forgetfulness," incompetence, and the like occur unintentionally, such defects can only be disclosed and reproved; but, in a strict sense, they cannot be criticized. Though I am sure that Dr. Timm did not want to imply this, his argument could well be picked up and used for immunizing strategies by irresponsible people inside and outside of the field. Moreover, I find the idea hard to accept that all the shortcomings in parapsychological research (or in research in other fields) could justly be attributed to mere "forgetfulness."

Ad 1: I would be most interested to learn how Dr. Timm knows that "psi phenomena ... cannot be explained within the framework of the established sciences." I think that, even if the parapsychological evidence were accepted as conclusive, this question still remains an open one.

Ad 2: Confer with section III. 3.

Ad 3: Dr. Timm is right in what he says about the "motivational function" of spontaneous paranormal occurrences. But, he will have realized that I did not express objections against this. However, I do not agree with his assessment of the chair tests with Gerard Croiset. I still see a lot of problems with these tests, especially as far as splitting of Croiset's statements is concerned. Moreover, quantitative analyses of such tests are not completely independent of prior subjective interpretation. In short: after Hoebens' recent substantial criticism of the celebrated Pirmasens chair test, I am more than reluctant to accept any claim based on tests of this kind.

Ad 5: I consider Dr. Timm's hypotheses that "stochastic laws" are responsible for as yet inconclusive results in psi research and that "monocausal interpretations" are insufficient, as entirely legitimate. Quite a lot of further research is needed to be sure about that.

Ad 6: I agree with Dr. Timm that it should not make a principal difference whether parapsychologists respond to inside or outside criticisms. Confer with the first part of my response to Dr. Stokes.

#### Response to Jerome Tobacyk

Professor Tobacyk restricts himself to commenting on just one of my recommendations. I agree with him that all the aspects he mentions might contribute to the revolutionary outlook among parapsychologists. And it may also be true that the need to view oneself (or to be viewed by others) "as a hero" may be one of the mainsprings of the lives of many people. I doubt, however, that this is generally so, since, if he were right, the world would be over-crowded with people claiming to be revolutionaries.

Moreover, what could the term "revolutionary" possibly mean if it applied to every human being? This term would become totally inappropriate for drawing any distinction. Now, it is striking and makes Professor Tobacyk's thoughts much less acceptable to me, that neither archeologists, nor classical philologists, nor even computer scientists are claiming to be revolutionaries or to be engaged in a revolutionary field of research. Obviously, parapsychologists are the only scientists pleased to call themselves (or their field) that. It is true that some physicists have also occasionally advanced this claim (and I do not approve of their self-assessments, either); but they have never pretended that being a physicist was equivalent with being a revolutionary.

#### Response to Rhea A. White

Ms. White is quite right: we could not possibly disagree more. We have been discussing some of the matters at issue in private communications since March, 1980, and it appears that we did not succeed in convincing each other of the adequacy of our respective opinions. However, we have never had any problems "to build a bridge" as far as our personal relations are concerned; and I am still confident that at least with respect to some scientific matters, it will not be impossible to build another one.

Ms. White's comments on my first recommendation run as follows: (1) parapsychologists use orthodox scientific research methods; (2) application of these methods leads to more or less impressive and valid results; (3) these results will contribute to the fact that parapsychology will eventually revolutionize scientific method in all fields.<sup>65</sup> I find this completely unacceptable. If (1) were false, the whole argument collapses. If (1) were true (and I am sure it is at least in many parapsychological investigations), then this implies that parapsychologists check the truth of their scientific propositions against orthodox scientific standards which depend on orthodox scientific method. How should it then be possible, however, to use these propositions about the results to revolutionize that scientific method (ology) and the scientific standards upon which they themselves depend? In other words: parapsychological propositions can only be valid relative to the applied and commonly agreed-upon methods and the standards for the validity of scientific propositions (and Ms. White herself seems to acknowledge this under (1)). Using these propositions for abolishing these methods and standards necessarily implies that at the same time the truth-conditions according to which the validity of these propositions was determined and, hence, these propositions themselves are also declared invalid! That is to say, that at the same time the new, "revolutionary" methodology is installed, the reasons for and the justification of this "revolution" are invalidated. This is simply because scientific standards or methods are normative sets of rules as to how to conduct or evaluate research. And, by definition, norms or rules cannot be disproved by empirical research. (Also confer with my response to Dr. Inglis).

For a response to Ms. White's comments on my second recommendation and related matters, see section III. 3. I agree with her that "whether or not something is 'scientific' cannot be judged on the basis of subject matter but rather by methodology."

As to her comments on my third recommendation, my citation of Sybo Schouten was, of course, not meant to imply that he is an "exception." However, there is a considerable number of parapsychologists who claim to be able to prove that psi was involved in certain spontaneous cases.

Regarding Ms. White's comments on my fifth recommendation, again see my response to Dr. Inglis' first point and to her own first point.

As to my sixth recommendation: I fully agree to Ms. White's first proviso. Regarding her second proviso, I also agree that critics should have made themselves familiar with the relevant literature before advancing their criticisms. And, in principle, I also agree that "master violinists" are particularly qualified for criticizing other master violinists. However, you should not lose sight of the fact, Rhea, that even "average violinists ... [or] pianists or truck drivers or bakers or sociologists" are well able to realize if those damned instruments are out of tune! Evidently, I disagree with Ms. White, that "the balance should be righted by leaning in the opposite direction from that proposed by Hövelmann." Moreover, I completely fail to see why even constructive criticism "can only be so in a negative sense" (my underlining).

Ms. White's comments on my final recommendation embody two apparent misconceptions of what I wrote: (1) I did not object to "reading the literature of and listening to exponents of the fringe groups." Of course, I myself read this literature, and I personally know a lot of such "exponents." I merely recommended not to meddle with them or to let them intrude into our field. Moreover, Ms. White holds that those people "may be practitioners of genuine psi." Of course, they may. But if we want to know if they really are, their apparent abilities must be subjected to scientific tests. I likewise do not have any objections against obtaining "suggestions and clues ... from these people that can be tested experimentally." (2) I do not believe that the "canon on scientific method is closed." That would be balderdash, indeed. Quite to the contrary, I believe that it can be demonstrated that this canon can never be closed, on principle. Therefore, I fully agree with Ms. White that scientists should always try to improve scientific methodology. Although I disagree with several things Ms. White says also with regard to this final recommendation, both these misconceptions make me hope that at least with respect to these questions we are not as far apart as it might appear at first sight. I would be pleased if this can serve as basis for building a foot-bridge at least.

#### Response to Leonard Zusne

I was a bit amused realizing that, while Dr. Stokes ascribes to me the metaphysical predilections of the Randi group, Professor Zusne believes that (at least after reading my paper carefully) I can be unquestionably identified as a parapsychologist. "Hövelmann stands with both feet planted firmly in parapsychological soil," he writes. It is about time now, I think, to briefly explain where my feet are "planted firmly."

I have always been extremely unsatisfied by the sheep/goat dichotomy, at least as far as the classification of parapsychologists and critics is concerned (also cf. the first part of my response to Dr. Stokes). Nevertheless, I have each time put my mark into the "sheep" column whenever

questionnaires or other inquiries were sent to me, partly in order to avoid the negative connotations with which the term "goat" is undoubtedly associated (this term is frequently used to denote "a-priori disbeliever" rather than "agnostic" in these contexts). I feel forced to admit that the best of parapsychological experiments have produced impressive results, and it appears extremely unlikely and implausible to me that explanations in terms of malobservation, gross incompetence, statistical errors, experimenter influence, fraud by subjects or experimenters, sensory leakages, or any other alternative explanation hitherto advanced by critics from inside and outside the field could (singly or jointly) sufficiently account for all of these results. Therefore, I regard it a convincing argument that we might have to look for some other explanation that is suitable for accounting for all the data which are as yet available in the parapsychological literature. I would not have serious objections against the use of the term "psi" to denote the unknown factor responsible for these results, although there might be more appropriate ones. But I hasten to add that, for me, the question remains a completely open one whether this factor "psi" will eventually turn out to be a "paranormal" (i.e. a hitherto unknown) one or whether it will prove to be a "normal" (i.e. an already known) one which, so far, has been overlooked, remained unconsidered (for whatever reasons), or erroneously put aside as irrelevant. These are the reasons (besides those which I have outlined in my response to Dr. Stokes), why I regard the "parapsychological: and the critical" position with respect to parapsychological findings and theories (not, however, with respect to the question of the legitimizability of parapsychology in general) as in principle equally legitimate, reasonable, rational and justifiable. It is for the same reasons that I would prefer to call both "parapsychologists" and "critics" (in the traditional sense) "parapsychologists."

Now, Professor Zusne claims that the mental attitude towards parapsychology is a function of the individuals' respective world views and, accordingly, he distinguishes between the "demonstrative" and the "dialectic" views of the world.<sup>66</sup> At first sight, this distinction seems most interesting and appropriate. On a closer inspection, however, it becomes clear that it is untenable, at least in the form in which it is presented in Professor Zusne's papers referred to in the most recent note (that is, if it is intended to be applied in such a way as to ascribe the "dialectic" world view to parapsychologists and the "demonstrative" world view to the critics). And this disjunctive classification seems to be what Professor Zusne had in mind: "It is this world view that produces the phenomenon of a parapsychologist and a skeptic looking at the same experimental results but with both arriving at diametrically opposed interpretations of what was observed," he writes in his comment. Then he proceeds to ascribe to the parapsychologist "the underlying predisposition to embrace ... a dualistic world view in which the customary laws of causality may not always operate." He then lists up four attitudes which he believes obtain as a consequence of the above-mentioned view of the world. Similarly, in his Perceptual and Motor Skills article, he writes:

"Paranormal research is usually instigated by a person who, consciously or not, is already predisposed to believe in the reality of paranormal phenomenon and will prefer explanations of such phenomena that are either rooted in the supernatural ... or else invoke known principles of nature in unspecified or unrecognized modes of action. In all cases, the dualistic tendency to reify the subjective is recognizable and constitutes the principle reason why the researcher engaged in paranormal research in the first place"<sup>67</sup>

"... the research is to prove a particular ideological point: that humans have souls, that these souls survive the death of the body, that telepathy, etc. are fact and that therefore science's view of human being and the world is invalid."<sup>68</sup>

"... parapsychological researchers typically have a world view to defend and... their research is not the natural outcome of activity in a field of science but one prompted principally by their desire to lend scientific respectability to ... a 'prepossession.'... One view places an emphasis on and values the subjective side of life, inner experience and thought, whereas the other stresses that which is tangible, objective, and deals with empirical facts"<sup>69</sup> (my underlining).

I find this version of Professor Zusne's distinction (which I would like to call the "dichotomy version") quite unacceptable (see below). However, there also exists a modified and weakened version of his distinction which seems useful to me. In a letter to me, Professor Zusne quoted from a letter he wrote to Professor Truzzi:

"... the dialectic/demonstrative typology is not a typology at all but a continuum. People are not either 'sheep' or 'goats,' but mostly hybrids with different amounts of 'sheepness' or 'goatness' in them. Furthermore, being a 'sheep' or a 'goat' represents a syndrome of characteristics. While these may be measured and represented by a single number, two individuals with the same measure may differ quite a bit in terms of the particular compositions of their respective syndromes. Second, I am particularly interested in the individual in whom the dialectic/demonstrative beliefs or forces are of about the same strength. The best example of this kind of person is William James. I have a hunch that the field of the 'anomalists' may be constituted largely of such individuals. This would explain why people like Hovelmann may sound like 'sheep' to one set of listeners and like 'goats' to another. They certainly adjust the tone of what they say according to their audience -- not for any machiavellian reasons, mind you, but because they can see things two different ways, although not necessarily at the same time"<sup>70</sup> (my underlinings).

As indicated above, Professor Zusne has changed his dialectic/demonstrative dichotomy into a "continuum" or a "composition of ... syndromes." I find this position much easier to accept than the "dichotomy version" (although there are still some objections possible). Undoubtedly, many people in the field of parapsychology come close to his original description of the "dialectic" world view (and some of them have participated in this dialogue); that is, their attitudes are more or less adequately described by the four points Professor Zusne lists up in his comment or by the quotes from his Perceptual and Motor Skills article. In contrast to the "dichotomy version," the "continuum version" can better account for decidedly undecided, agnostic positions like mine.

I would certainly most strongly object against any attempt, however, to argue that the parapsychologists reveal such attitudes as those mentioned under the "dichotomy version," or that the "dialectic" view were at the roots of any engagement in this field. I can categorically deny that Professor Zusne's distinction of predispositions (in the "dichotomy version") applies in my case. I simply do not have any preference, neither for the "paranormal" nor for the "normal" solution with regard to the problem of the as yet unknown factor responsible for the outcomes of some of the parapsychological investigations. That is, I would be satisfied with both solutions to quite the same degree. I do just have one preference: I wish to see a solution at all, whatever it might be! And I know for sure that there are a number of parapsychologists (though too few still) who share this attitude (also some of these people have participated in the current dialogue). The above are the reasons -- and here, again, I fully agree with Professor Zusne -- why I think that parapsychologists have much to potentially gain also by cooperation with the anomalistic psychologists within psychology. And this is, moreover, fully in line with Dr. Blackmore's request that we study paranormal phenomenon scientifically wherever that may lead. Even if, in the most extreme case, parapsychology became superfluous as a separate scientific field in the long run. However, I strongly object, to repeat this, against calling the "dialectic" view typical of the parapsychologist.

To conclude, I would like to thank Professor Zusne for his highly stimulating comments on my paper which provided me with the welcome opportunity to try to point out where my feet are "planted firmly."

### Conclusion

The "commentaries ... are of an amazing variety," I wrote in the introduction. Indeed, while some commentators commended what they flattered me by calling the "farsightedness," "relevance," "reasonableness," or "eloquence" of my recommendations, others found them rather "obscure" (at least in part), "exaggerated," "absurd," or "eccentric." Some even thought that the whole thing might be an attempt at providing parapsychologists with some rhetorical tricks for more effectively throw-dust into the critics' eyes. It is evident from all these responses,

that the apprehensions of what science, parapsychology, and criticism are or should be are extremely diverse among "parapsychologists" as well as among "critics" (and also -- though probably to a lesser degree -- between these groups). This clearly shows that some sort of agreement on these questions is not so much an achievement of the past as a task for the future<sup>71</sup>. I would welcome if such an agreement were sought to be reached in a rational and reasonable way across the unpleasant demarcating line rather than on both sides separately. I am rather confident that this can be achieved because there are some vague but promising signs (in this dialogue as well as elsewhere) indicating that this demarcating line is already becoming permeable here and there.

Finally, I wish to thank Professor Marcello Truzzi for so kindly making considerable parts of issues 11 and 12 of Zetetic Scholar available to us. As for me, I found this zetetic dialogue highly rewarding. I am not entitled to speak for my commentators, but I am inclined to assume that at least some of them will agree with me.

### III. SOME FURTHER ATTEMPTS AT CLARIFICATION

#### 1. Revolution versus Foundation and Justification<sup>72</sup>

Because the deductive model of foundation and justification (as advocated by Popper and other philosophers) must necessarily run into insurmountable difficulties with respect to the question of the foundation and justification of the deductive model itself, Kuhn's<sup>73</sup> concept of scientific developments and scientific changes has become most influential in philosophy as well as in many branches of science. Since I assume that readers of Zetetic Scholar are quite familiar with the Kuhnian conception, I need not go into its details here. Suffice it to say that, according to Kuhn's historicistic views, theories and entire scientific branches develop beyond and quite irrespective of any attempt at justification. The Kuhnian conception which I find quite inadequate has also been accepted by most parapsychologists since it is thought to adequately reflect their own situation vis a vis the scientific community. So, this conception serves (or may be serving, at least) the following functions (among others) in our field: (1) Kuhn's opinions are believed to be picturing a pathway to future legitimacy; (2) the Kuhnian conception may serve as a welcome excuse for the fact that parapsychology has hitherto failed to gain scientific recognition; (3) this conception seems to dispense from the obligation to advance tightly reasoned propositions, since scientific changes must no longer be regarded a question of foundation and justification but one of social power. Proper justification can thus be viewed as secondary or even unnecessary.

As a consequence of Kuhn's conception, foundation and justification of scientific propositions are replaced by science-sociological and science-historical reflections and by a recourse to factual scientific developments. This is surprising, at first sight, since "foundation" and "justification" traditionally were supposed to mean methodical rather than historical securing of scientific propositions. According to Kuhn's conception which, strictly speaking, is nothing but a highly intelligent excuse for the failure of the deductive model of

justification, valid foundation and justification do no longer exist in science. Questions as to whether a given theory is a good and sound one are now answered by referring to the fact that -- after a "revolutionary" breakthrough that led to a "paradigm switch" -- this theory did factually prevail (or fail, for that matter). A look at philosophy and the sciences suggests that foundation and justification have already been given up to a certain degree in favor of mere analyses of factually existing theories and factually occurring scientific changes which seem to gain the character of natural necessities.

Now, the Kuhnian conception was only advanced to cope with the problems of the deductive model, and the fact has been completely lost sight of that there is another model of justification and foundation which does not run into the difficulties which confronted the deductive one. This conception is able to provide proper foundation and justification of scientific propositions and, in addition, can provide a legitimizable basis for foundation and justification by way of a recourse to elementary practices of predication and action. I deeply regret that it is quite impossible here to explain this model adequately, since this would take too much space. Therefore, I am forced to ask readers to have patience until my detailed paper (see note 72) will be available. For first information one may look up some of the literature given in note 28.

A science-historically oriented practice in the Kuhnian sense is continuously in danger of becoming a stylish but cheap defense of poor science. If the quality of a theory is not judged by its foundation and justification but rather by the way it has factually superseded competing ones, then a theory can be regarded as a proper one for the mere reason that it superseded rivalling ones. Consequently, foundation and justification are viewed as mere historical coincidences. Systematic and well-founded argumentation which once has been obligatory in cases where the validity of theories was at issue, comes down to the mere historiography of scientific developments.

The above-mentioned alternative model allows for a critical reconstruction of the justificatory steps of a theoretical development, instead of merely looking at the results of such a development. Among many others, this view implies the following important advantages: (1) a very essential part of human activity need not be conceived as ruled by historical constraints; (2) unfounded parts of theories can be distinguished from well-founded ones irrespective of whether or not this theory is a commonly accepted one; (3) faulty developments can be characterized as such, which proves impracticable if all possible developments of scientific theories or branches are taken to be equally valid in principle; (4) central parts of predominating theories and practices can be substantially criticized and improved without being forced to wait until the adherents of these theories have died out; etc. All this enables scientists to be critical of their own practice and to change it where this may turn out to be desirable and to justify their requests that others give up or modify their respective ("orthodox scientific") theories if this can be shown to be really necessary. Therefore, especially parapsychologists should stop talking about "revolutions" and care more about the foundation and justification of their theories and practices instead. So much on the background of my recommendation to give up revolutionary pretention.

## 2. Terminology

Several commentators wondered how a methodically constructed parapsychological terminology could be brought about. And especially Dr. Palmer has communicated some highly relevant thoughts about the inadequate linguistic means of the field. As in the case of the above-mentioned model of justification, it is hardly possible to provide a detailed outline of how, in my view, a methodical construction of an adequate terminology can be effected. I will perhaps take up Ms. White's suggestion to do this in a separate paper. However, the following sketch may well be sufficient to grasp the basic idea of how a scientific terminology should be constructed on the most fundamental level:

Since scientific propositions differ from sentences as they are produced in every-day life in that they are connected with special claims of validity, scientists are bound to be able to defend these propositions against possible objections. To be in a position to meet this obligation, their terminologies must only contain technical terms which have been given an explicit standardization. The standardization of a terminology which only consists of terms which have been introduced stepwise, complete, and free of circular definitions, is to be called a methodical construction of a terminology. Since every scientist, in the first place, is a member of a certain linguistic community which uses natural colloquial language free of explicit definitions, these linguistic means must be transformed stepwise into a more precise and reliable scientific language. For this purpose, terminological agreements with regard to the respective contexts and goals of scientific activities are required. Now, it is quite inadequate to begin this transformation with "definitions by replacement" in a logical sense. This was done by Logical Empiricists and is still occasionally considered the only possible way today; it can easily be shown, however, that such "definitions by replacement" (as well as so-called "implicit definitions") inevitably lead into a regressus ad infinitum. I would suggest, instead, that the transformation of ordinary into technical linguistic means should be guided by the purposes scientists pursue with their scientific actions, as for instance with the production of measuring tools in mechanics. By using examples and counter-examples for respective objects, situations, or processes, exemplary determinations of predicators for these objects, situations, and processes can be introduced. With examples and counter-examples, we may also determine the use of predicators for our own activities as scientists (e.g., for experimental actions). By means of such predicators, rules for predicators, and some logical rules and procedures for terminological determination,<sup>4</sup> terminologies can be methodically constructed which fulfill the the above requirements.

Problems are somewhat more complex if we subsequently try to standardize terms which are already commonly used in a branch of science. Besides the request to construct a methodical terminology, the problem of adequacy with regard to already existing theories of this branch of science is to be considered. If scientists want to conserve the number and substance of already existing and agreed-upon theories, while constructing a methodical terminology, adequacy of the new terminology must thus be considered both with respect to the requirements of a methodical terminology and with respect to the traditional way of talking

about the already existing theories. Of course, the reasons why it is believed that the number and substance of these theories should be conserved must be explicitly stated which, however, can already fail because of the lack of an adequate terminology. In such a case, these theories must be sacrificed. The only alternative would be to close one's mind to the requirement of a methodically constructed and explicitly standardized terminology. But this would mean, in the last resort, that the means for distinguishing between scientific propositions and unscientific ones are renounced deliberately. Scientists who prefer to accept the latter possibility have overlooked that "objectivity" or "intersubjectivity" are themselves predicators for propositions (and not for the quality of research work; see again Dr. Scott's comments and my response). That is to say, that objectivity or intersubjectivity cannot be reached independent of language but must start with the objectively or intersubjectively comprehensible use of terms.

Anyway -- if we decide to do scientific work, rules for the use of technical terms in a scientific practice must be explicated, and the standardization of the required terminology must make use of exemplary determinations of predicators by means of examples and counter-examples.

### 3. Can Science "Explain the World," And Can It Solve the Survival Problem?

I would like to start with a very brief discussion of the claim (advanced by several commentators) that science, and physics in particular, is to explain the world and the universe and what not.

It is evident to everybody, I think, that, for pragmatic reasons, natural scientists are unable to investigate or explain "the world" or "the universe" at a single try. Therefore, the main task of science, namely to "explain the world," we are told, is divided into a great number of manageable sub-tasks which are called "experiments." Now, any experiment, say, in physics (upon which we will concentrate, since it is the "paradigmatic" field to solve the "problem" in question) can be described as a system of relatively low complexity. Such a system is defined by a set (= a finite number) of elements and a set (= a finite number) of relations between these elements, and it can be described by means of a differential equation. This implies, moreover, that each system of this kind has well-defined boundaries. That is to say, (1) that there are both elements which are parts of the system and other elements which are not parts of the system (otherwise physicists would, indeed, be investigating the whole "world" or "universe" at a single try), and (2) that for any given element it can be decided whether or not it is a part of the system. It is evident, that a physical experiment can only produce scientific knowledge about those elements which are parts of the system, but not about outside elements. The same is true for 2, 3, 4, ... , n experiments no matter whether they are considered singly or jointly. That is to say, even by means of physical experimentation it will never be possible to explain "the world" or "the universe," since there will always remain outside elements uninvestigated. Therefore, any such claim is untenable. It is a myth.

To turn to the survival problem, I have several fundamental objections against this kind of research. Obviously, I have failed, however, to

convince anyone with what I wrote in my paper. At least, commentators almost unanimously disagreed with me on this question. I would question the usefulness of survival research on (a) pragmatic, (b) mental-hygienic, and (c) language-philosophical grounds. Furthermore, I would object to survival research for several other reasons which are partly related to conceptions of scientific explanation, scientific hypothesis testing, the general competence of science, etc. To explain even one of these additional objections would require much more space than is available here. I will do that on other occasions. Since I have already tried to explain my objections based on (a) and (b) in my original paper, I will not repeat these arguments here. Instead, I will try to explain my objection (c).

In my view, one of the main reasons for the futility of survival research is the following: in section III. 2 we have seen that, in order to construct a scientific terminology that enables us to distinguish strictly between sound and well-founded propositions as well as structured groups of propositions (i.e. theories) on the one hand and unfounded, metaphysical, and unscientific ones on the other, we must start with exemplary determinations of predicators to denominate those objects, situations, or processes which are to be described, explained, investigated, etc. For the introduction of such predicators, we need examples and counter-examples of such objects, situations or processes which are more or less readily at hand and disposable.

Thus a physicist, after having performed certain experimental actions, can point at certain features of his measuring protocol and say: "That's what we want to call the factor 'gravitation,' henceforth (whatever its interpretation)." Counter-examples are readily at hand, and research goes on. Similarly, a parapsychologist, after having performed certain experimenting actions,<sup>75</sup> can point at a certain feature of his experimental protocol and say: "That's what we want to call the factor 'psi' henceforth (whatever its interpretation, normal or paranormal)." Counter-examples are, in principle, at hand, and research goes on. (Both of these introductions have in common that they are made on an operational basis). A survival researcher, after ... What could he have done, and what should he point at to call it "survival"? The problem is that there are no such examples and counter-examples which could be chosen to methodically introduce the predicator "survival." That is, the survival researcher does not dispose of any object, situation or process that could be counted as an example or counter-example of "survival," since the operational basis is lacking. There are two possible (but treacherous) strategies he might choose: (a) he might point at certain features of findings of research in mediumism, reincarnation, etc.; or (b) he might invent examples, say, of an individual who did survive and of another individual who did not survive, call one of these "survival," and -- research does not go on (or, at least, need not go on), since his strategies have produced a couple of insurmountable difficulties:

(1) To be able to formulate the examples he invents under strategy (b), he already needs the predicators "survival" and "non-survival" (or equivalent ones). That is, the very terms he wanted to introduce methodically by way of an exemplary predication have already been used for providing the preconditions for this predication. In other words: in order

to get to the means which would enable him to decide what should qualify as "survival" and what should qualify as "non-survival," this distinction must already have been made beforehand. This circularity is inescapable.

(2) By means of further research, the physicist is able, in principle to find out what it is that he has termed "gravitation." Likewise: by means of further research, the parapsychologist is able, in principle, to find out what it is that he has termed "psi." The survival researcher, however, is not able to find out what it is that he has termed "survival," since, regardless of whether he has chosen strategy (a) or (b), he has already committed himself to an interpretation of what he has termed "survival." His further research can only produce analytic truths because of his very introduction of the term "survival." He cannot obtain any proof of survival that is not yet predetermined by his introduction of that term. If, in his further research, the survival researcher happens to come across a case suggestive of survival, there are only two alternatives left: if, relative to his introduction of that term, he classifies his new case as a genuine instance of survival, then his assessment of genuineness is true by definition. And if this new case does not qualify as a genuine instance of survival (in terms of his introduction), the researcher is unable to say anything of value about this case (at least as far as its relevance to the survival hypothesis is concerned). That is, any case he might consider is an instance of genuine survival (a) by definition or (b) not at all.

A slightly different version of this objection is that survival research is necessarily performed by living human beings. However, by definition, living human beings (no matter whether they are survival researchers or mediumistic mediums or supposedly reincarnated children) can not know by experience what dying or what 'life after death' (whatever that means) is like. To be able to form a judgement about the validity of claimed data "suggestive of survival" (e.g., of descriptions of "life after death" obtained, say, by way of mediumistic communications), these living researchers must dispose of a pre-specified set of criteria as to what to accept and what to reject as evidence for survival. But how could these living researchers possibly be able to establish such a set of criteria? The only conceivable way would be to already base this establishment on data "suggestive of survival," that is, on the very data the validity of which they want to assess by means of these criteria! This proceeding is clearly circular, and no scientific value, whatsoever, can be justly claimed for any results obtained that way. Strictly speaking, it is not even possible to state which kinds of data do (or do not) qualify as "suggestive of survival."

From all this it follows that empirical scientific knowledge about survival is impossible, on principle. That is to say, it is not possible to form propositions about the possibility or impossibility of survival which could be claimed to be "scientific" in any conceivable sense of that term.

I trust that in spite of my insufficient command of written English, the gist of my argument has become clear. Now, I do not mind, of course, if people talk about belief or disbelief with regard to the question of survival. This way of talking, in the last resort, depends on how they

wish to talk. And, in the ideal case of a "conscious" way of living one's life, wishes are acts of freedom. So, anyone must decide for himself which position he prefers to adopt for a non-scientific way of talking about survival. However, I neither see a possibility of deciding scientifically whether survival is possible, nor can I even conceive of a way to methodically introduce the linguistic means necessary for talking about survival in a scientifically justifiable manner.

#### NOTES AND REFERENCES

- 1 West, D.J., "Thoughts on testimony to the paranormal", in: Parapsychology Review 13 (1982), 5, 1-8 (quoted from p. 8).
- 2 Hoebens, P.H., "Cambridge Centenary of Psychical Research: critics heard, encouraged to cooperate", in: Skeptical Inquirer, 7 (1982-83), 2, 2-4 (quoted from p. 4)
- 3 To avoid possible confusion: the term "science" is meant throughout this paper to comprise both the natural sciences and the social sciences or humanities.
- 4 That is not to say, that present-day science is necessarily sacrosanct and unimpeachable per se. Parts of the practices of any branch of science can be justly criticized, and I have done this in my writings on science-theoretical problems of physics, biology, and linguistics.
- 5 Evidently, there is an additional problem in astronomy (and a similar one in geology), for no one could justly claim to be able to "produce" a solar system (or certain strata in the earth's crust). "Explanations" in astronomy (geology) are accepted as adequate if we are able to produce an idealized technical model that is suitable to simulate, say, the movements of the planets (or tectonic movements) on a small scale.
- 6 Of course, much more is to be said on explanation. I am planning to do so in a paper on scientific explanation in science in general and in parapsychology in particular.
- 7 Smith, H.A.: "Presidential address", in: Proceedings of the Society for Psychical Research, 24 (1910); 330-350. (quoted from pp. 338-339.
- 8 Ibid., p. 341.
- 9 Cf., Hovelmann, G.H., "On 'Hume's fallacy,'" in Journal of Parapsychology, 45 (1981), 367-369.
- 10 Zorab, G., "The forlorn quest" Paper presented at the 25th Annual Convention of the Parapsychological Association, Cambridge, U.K., August 1982.
- 11 A version of this recommendation is also to be found in Blackmore, S.J., "Parapsychology -- with or without the OBE?" in: Parapsychology Review 13 (1982), 6, 1-7. Also cf. Dr. Blackmore's provocative paper: "Unrepeatability: parapsychology's only finding," presented at the 32nd International Conference of the Parapsychology Foundation, San Antonio, October 1983, which I consider extremely important for the questions discussed here.
- 12 James McClennon is probably not completely wrong when he claims:  
"The nature of support that proponents of rejected anomalies are forced to seek in order to continue their research endeavor ensures the anti-scientific direction that the research generally takes. The history of parapsychology illustrates this point. Whenever a 'discovery' within the field seems to have a mechanistic explanation, that element is stripped, deemed unsuitable for the psychic researcher."



Cf., McClenon, J., Rejected Anomalies and Deviant Science. Unpublished manuscript, 1982 28 pp. (quoted from p. 20). An earlier version of this paper was presented before the February 1982 SERPA Conference under the title, "Psi as a rejected anomaly: social patterns surrounding deviant sciences."

- 13 Truzzi, M., "Editorial", in Zetetic Scholar, #10 (1982), 5-6 (quoted from p. 6). A more detailed discussion of this question is to be found in Hövelmann, G.H., "Some reflections on the term 'paranormal,'" in Journal of Parapsychology, 47 (1983), in press.
- 14 Dr. Blackmore quotes from a paper by Leonard Zusne on anomalistic psychology with which I am going to deal below along with my responses to Professor Zusne.
- 15 Cf., Hövelmann, G.H., "Zum Problem der Wiederholbarkeit parapsychologischer Experimente. To appear in Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie, 25 (1983), in press. An abridged English version of this article is to appear -- under the title, "Are psi experiments repeatable?" -- in the Research Letter of the Parapsychology Laboratory at the University of Utrecht, 1983 (These papers also contain a distinction between "experimental" and "experimenting" sciences).
- 16 Galilei, G., Dialogue Concerning the Two Chief World Systems -- Ptolemaic & Copernican. 2nd revised edition. Berkeley & Los Angeles: University of California Press, 1967; Kepler, J., Gesammelte Werke. Band I-XIX. Munich: C.H. Beck, 1938-1975; Copernicus, N., De Revolutionibus Orbium Coelestium Libri, 6. Nuremberg, 1543.
- 17 Kuhn, T.S., The Copernican Revolution. Planetary Astronomy in the Development of Western Thought. Cambridge, Mass. & London: Harvard University Press, 1957 (quoted from p. 135).
- 18 Ibid., p. 181.
- 19 Zilsel, E., "Copernicus and the mechanics," in Journal of the History of Ideas, 1 (1940), 113-118.
- 20 Toulmin, S.E., Human Understanding. Volume I: Concepts, Their Collective Use and Evolution. Oxford: Clarendon Press, 1972 (esp. chapt. 1.4).
- 21 Eysenck, H.J., Letter to the author, dated March 3, 1983.
- 22 The only really balanced account of Project Alpha I have yet come across is that by Broad, W.J., "Magician's effort to foil scientists raises questions," in The New York Times, February 15, 1983, pp. 19 and 21.
- 23 Besides Randi's Advisory Notices I and II and his Press Conference Statement, especially cf. the notorious (unsigned) article "Psychic abscam" in Discover, March 1983. (Postscript to this note; May 21, 1983: I just received a mailing from the Parapsychological Association on Project Alpha. I disagree with a few of Peter Philipps' opinions as expressed in his "brief report." I regret that the P.A. considered it necessary to include Rockwell's ad hominem statements with its mailing.

(Second postscript to this note; June 24, 1983: I regret to say that I am extremely disappointed by the recent accounts of Project Alpha by Martin Gardner and James Randi, respectively. Both contain demonstrable inaccuracies and, again, gross exaggerations of the implications of the findings. What is really regrettable about this way of presentation is that Randi himself provides his critics with the pretext to dismiss his findings in toto and to cheerfully ignore the project's vital lessons. Thus, I cannot help concluding that, in some sense, Randi's way of presenting his results is rather instrumental in bringing about what he wants to criticize. Cf., Gardner, M., "Lessons of a landmark PK hoax,"

in Skeptical Inquirer, 7 (1983), 4, 16-19, and Randi, J.: "The Project Alpha experiment. Part 1. The first two years," in Skeptical Inquirer, 7 (1983), 4, 21-33.

(Third postscript to this note; November 3, 1983: In the meantime, Randi published the second part of his report on Alpha: Randi, J.: "The Project Alpha experiment: Part 2. Beyond the laboratory," in Skeptical Inquirer, 8 (1983), 1, 36-45. It is much more balanced than the first part, although I still have several objections to it. In "James Randi und Projekt Alpha--oder: wie leichtgläubig sind 'die' Parapsychologen?" in: Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie, 26 (1984), in preparation, I attempt a rational assessment of the whole affair (a more detailed English version is also in preparation).)

- 24 Cf. Bierman, D.J., "Hoe wetenschappelijk zijn onze dritici?" in Tijdschrift voor Parapsychologie, 50 (1982), 137-142; also cf. the response by Hoebens, P.H., "De balk in eigen oog," scheduled for publication in the Tijdschrift voor Parapsychologie.
- 25 Cf. Hövelmann, G.H., "Paradigm rediscovered?--oder: Schamanismus total," in Duerr, H.P. (ed.), Unter dem Pflaster liegt der Strand. Band 12. Berlin: Karlin Kramer Verlag, 1983, pp. 176-187; Bauer, E. Hövelmann, G.H., & W. von Lucadou, "Der Jahrhundertkongress -- Cambridge 1982," in Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie, 24 (1982), 193-215.
- 26 Inglis, B., "Power corrupts: skepticism corrodes," in Roll, W.G., Beloff, J. & J. McAllister (eds.), Research in Parapsychology 1980. Metuchen, N.J. & London: Scarecrow Press 1981, 143-151.
- 27 Again cf. my papers referred to in note 15.
- 28 Only few accounts are available in English; but cf., for instance, Janich, P., "Physics -- natural science or technology?" in Krohn, W. et al. (eds.), The Dynamics of Science and Technology. Dordrecht: D. Reidel, 1978, 3-27; Janich, P., Protophysics of Time. Dordrecht: D. Reidel, in preparation; Lorenz, K., "The concept of science. Some remarks on the methodological issue 'construction' versus 'description' in the philosophy of science," in Bieri, P., Horstmann, R. -P. & L. Kruger (eds.), Transcendental Arguments and Science. Dordrecht: D. Reidel, 1979, 177-190; Lorenz, K., "Science, a rational enterprise? Some remarks on the consequences of distinguishing science as a way of presentation and science as a way of research," in Hilpinen, R. (ed.), Rationality in Science. Dordrecht: D. Reidel, 1980, 63-78; Lorenzen, P., Normative Logic and Ethics. Mannheim & Zurich: Bibliographisches Institut, 1969; also cf. Kamlah, W. & P. Lorenzen, Logische Propädeutik. Vorschule des vernünftigen Redens. 2nd improved and enlarged edition. Mannheim, Vienna & Zurich: Bibliographisches Institut, 1973; Mittelstraß, J., Die Möglichkeit von Wissenschaft. Frankfurt/M.: Suhrkamp, 1974. Also cf. the literature referred to in my papers quoted in note 15. An extensive English anthology on the philosophy of the "Erlangen School" is now in preparation and will probably be available in 1984 or 1985.
- 29 Keil, H.H.J., "Parapsychology -- searching for substance beyond the shadows," in: Australian Psychologist, 15 (1980): 145-168 (quoted from p. 163).
- 30 Cf. Roll, W.G., "The changing perspective on life after death," in Krippner, S. (ed.), Advances in Parapsychological Research. Volume III. New York & London: Plenum Press, 1982, 147-291, as well as my review of this volume in Theta, 11 (1983), in press.

- 31 Sheldrake, R.: "Morphic resonance, memory and psychical research," in Parapsychological Journal of South Africa 3. (1982), 2, 70-76; Roukens de Lange, A., "Evolution and formative causation," in Parapsychological Journal of South Africa 3 (1982), 2, 84-105.

(Postscript to this note; November 3, 1983: I still haven't read Sheldrake's book, but I have seen Stephen Braude's essay review of it (cf. Braude, S.E.: "Radical provincialism in the life sciences: A review of Rupert Sheldrake's A New Science of Life," in Journal of the American Society for Psychical Research, 77 (1983), 63-78). On the assumption that Braude adequately represents Sheldrake's opinions in his review, I find his criticisms (with very few exceptions) convincing.)

- 32 Krippner, S., "Editorial," in Psychoenergetic Systems, 2 (1977), 5-11 (esp. p. 10).
- 33 Rao, K.R., "On the question of replication," in Journal of Parapsychology, 45 (1981), 311-320.
- 34 Allison, P.D., "Experimental parapsychology as a rejected science," in Wallis, R. (ed.), On the Margins of Science: The Social Construction of Rejected Knowledge. (Sociology Review Monograph 27). Keele, Staffordshire: University of Keele, 1979, 271-291; also cf. Allison, P.D., Social Aspects of Scientific Innovations: The Case of Parapsychology. Unpublished Master's Thesis. University of Wisconsin, 1973.
- 35 Allison, P.D. (1979; in note 34): p. 287.
- 36 Ibid., p. 288.
- 37 For instance, cf. Hövelmann, G.H., "Technikorientierte versus welbilde-erzeugende Naturwissenschaft. Marginalien zu einigen Auffassungen W. Buchels," in Zeitschrift für allgemeine Wissenschaftstheorie/Journal for General Philosophy of Science, 15 (1984), in press.
- 38 Hövelmann, G.H.: "Review of John L. Randall: 'Parapsychologie und die Natur des Lebendigen,'" in Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie, 23 (1981), 123-127; Tetens, H., "Organisation und Information. Über eine methodologische Legende in der Biologie." Paper presented at the 15th Workshop on Phylogenetics and Systematics. Biebergermünd im Spessart, May 1982.
- 39 For a discussion of the relations between "parapsychologists" and "critics," see my responses to Dr. Stokes and Professor Zusne.
- 40 I would indeed prefer "laws of science" instead of "real world principles."
- 41 Anonymus: "Ermittlungen hinter Nebelvorhängen," in Die Polizei-Zeitung Baden-Württemberg, 1981, No. 4, p. 3.
- 42 Wimmer, W., "Die merkwürdige Wissenschaft der Spukprofessoren," in Kriminalistik, 24 (1970), 329-338; Wimmer, W., "Eine andere Wirklichkeit? Vom Unfug der Parapsychologie," in Deutsches Ärzteblatt, 71 (1974), 732-739; Wimmer, W., "Okkultismus und Rechtsordnung. Die Methoden der Parapsychologie in kriminalistischer und juristischer Sicht," in Archiv für Kriminologie, 164 (1979), 1-16; Wimmer, W., "Hexenwahn an Universitäten?" in Zeitschrift für Allgemeinmedizin, 56 (1980), 13900-1400.
- 43 Truzzi, M., "Reply to Christopher C. Scott," in Zetetic Scholar, #10, (1982), 152.

(Postscript to this note; November 3, 1983: Recently, Jérôme Tobacyk ("Reduction in paranormal belief among participants in a college course," in Skeptical Inquirer, 8 (1983), 1, 57-61) advanced claims very similar to those made by Professor Mertens, and he, too, did not quote any evidence to support them. Cf. my critical remarks on this aspect of

Tobacyk's paper in my "Letter to the Editor" in Skeptical Inquirer, 8 (1984), 3, in press. Also cf., Levis, K. (ed.), Violence and Religious Commitments: Implications of Jim Jones's People's Temple Movement, University Park: Pennsylvania State University Press, 1982.)

- 44 John F. Kennedy University. General Catalog 1982-1984. Orinda, Cal.: John F. Kennedy University Press, 1982 (quoted from p. 171).
- 45 Ibid., pp. 112 and 172.
- 46 Auerbach, L.M., "'Mentalism for Parapsychologists.' A report on an ASPR workshop," in ASPR Newsletter 9 (1983), 4.
- 47 Ibid.
- 47A On this occasion the Council of the Parapsychological Association adopted a resolution which was announced by its president, Stanley Krippner, in Marcello Truzzi's roundtable on "The role of conjurors in psychical research and parapsychology." This resolution reads: "Historically, parapsychologists have availed themselves of the services of experts in fields relevant to their own: statisticians, engineers, and --on occasion-- magicians. The P.A. welcomes collaboration with magicians who, by their past behavior and membership in respected organizations, have maintained high standards of professionalism and have adhered to the ethical code of the fraternity of magicians. We suggest that it is disadvantageous to both parapsychologists and conjurors to interact with magicians who do not meet these criteria and who would exploit such an interaction for personal gain. Therefore, the P.A. Council has voted unanimously to request from organizations such as the International Brotherhood of Magicians, Society of American Magicians, and the Psychic Entertainers Association a list of their members who, regardless of their opinions on the existence of psi, would be willing to consult with P.A. members regarding adequate controls against fraud. We look forward to a fruitful professional relationship with these individuals."
- On the relationship between parapsychologists and conjurors, also cf. Marcello Truzzi's excellent survey "Reflections on conjuring and psychical research" (to be published) and my reflections after the 1983 P.A. Convention (Hövelmann, G.H., "Einige Überlegungen nach der 26. Jahrestagung der Parapsychologischen Association 1983," in Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie, 25 (1983), in press.
- 48 Cf. Hövelmann, G.H., "Kooperation und Konkurrenz im wissenschaftlichen Schrifttum der Parapsychologie," in Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie, 22 (1980), 143-156; Hövelmann, G.H., "Cooperation versus competition: in defense of rational argument in parapsychology," in European Journal of Parapsychology, 4 (1983), 483-505.
- 49 Beloff, J., "Three open questions," in Parapsychology Review, 14 (1983), 1, 1-6 (quoted from p. 1).
- 50 Hövelmann, G.H., "Review of Gzara (cf. note 51)," in Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie 23, (1981), 253-259.
- 51 Gzara, M.: Scharlatanerie. Zur Sprache und Beweisführung pseudowissenschaftlicher Literatur. (Medical Dissertation). Marburg/Lahn: Institute for Forensic Medicine of Marburg University, 1980.
- 52 A brief account of our informal conference and its achievements is to be found in English in: Frazier, K., "Parapsychologists, critics agree to consensus statement," in Skeptical Inquirer, 7 (1983), 4, 4-6.

- 53 Some interesting thoughts on this problem are already to be found in: Mundie, C.W.K., "Confusion about 'ESP' and skepticism about ESP," in Angoff, A. & B. Shapin (eds.), A Century of Psychical Research: The Continuing Doubts and Affirmations. New York: Parapsychology Foundation, 1971, 17-30.
- 54 In the second of my papers referred to in note 48, I have tried to define "rational discourse."
- 55 For instance, cf. Collins, H.M., & T.J. Pinch, "The construction of the paranormal: nothing unscientific is happening," in Wallis, R. (ed.), On the Margins of Science The Social Construction of Rejected Knowledge. (Sociological Review Monograph 27). Keele, Staffordshire: University of Keele, 1979, 237-270; Pinch, T.J., & H.M. Collins, "Is anti-science not-science? The case of parapsychology," Nowotny, H. & H. Rose (eds.), Counter-movements in the Sciences. (Sociology of the Sciences, Vol. III). Dordrecht: D. Reidel, 1979, 221-250; Collins, H.M., & T.J. Pinch, Frames of Meaning. The Social Construction of Extraordinary Science. London: Routledge & Kegan Paul, 1982.
- 56 Beloff, J., "The finger-prints of psi." Paper presented at the 5th International Conference of the Society for Psychical Research, Bristol, April 1981.
- 57 Cf. my papers referred to in note 15; also cf. Hövelmann, G.H., "Reality relevance, and responsibility," in Zetetic Scholar, #10 (1982), 131-133; Hövelmann, G.H., "Review of 'Parapsychology and the Experimental Method' (ed. by B. Shapin & L. Coly)," in Journal of Parapsychology, 47 (1983), in press.
- 58 Cf. my paper referred to in note 37; also cf. Hövelmann, G.H., "Sprachkritische Bemerkungen zur evolutionären Erkenntnistheorie," in Zeitschrift für allgemeine Wissenschaftstheorie/Journal for General Philosophy of Science, 15 (1983), in press.
- 59 Therefore (and for several other reasons some of which concern the history of mathematics), I also disagree with Dr. Rosen's position as outlined in: "Wholeness and psi: the implications of David Bohm's concepts for parapsychology. Part I," in: Theta, 10 (1982), 74-78; "Wholeness and psi: The implications of David Bohm's concepts for parapsychology. Part II," Theta, 11 (1983), 2-8; and in "Psi-modeling and the psychophysical question: an epistemological crisis," in Parapsychology Review, 14 (1983), 1, 17-24.
- 60 Again, cf. my response to Mr. Hoebens as well as notes 22-24.
- 61 Although I greatly admire Dr. Palmer's contributions to parapsychology, I share Dr. Stokes' concern about the "astrological" part in his 1979 presidential address (cf. Palmer, J., "Parapsychology as a probabilistic science: facing the implications," in Roll, W.G. (ed.) Research in Parapsychology 1979. Metuchen, N.J. & London: Scare Press, 1980, 189-215 (esp. pp. 208-209). I also share Dr. Stokes' concern about recent requests made by members of the Parapsychological Association to give up rigid scientific methodology. For instance, cf. Gruber, E.R., "Inside and outside the paranormal," Paper presented at the 31st Annual International Conference of the Parapsychology Foundation, London, August 1982. For a criticism of such attempts see my papers referred to in note 25. Also cf. Hövelmann, G.H., "... not much of a scholarly piece?" in Duerr, H.P. (ed.) Unter dem Pflaster liegt der Strand. Band 10. Berlin: Karlin Kramer Verlag 1982, 184-198: a slightly revised version of this paper is to appear -- under the title "Parapsychologem und das Irrationale" -- in Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie, 25 (1983), in press.
- 62 Dessoir, M., "Die Parapsychologie. Eine Entgegnung auf den Artikel: 'Der Prophet,'" in Sphinx, 4 (1889), 341-344.
- 63 Ibid., p. 342 (my translation), also cf. my paper referred to in note 13.
- 64 Again, cf. my papers referred to in note 15.
- 64A Dr. Timm put the word "forgetfulness" in inverted commas. As I understand it, these inverted commas are to indicate that he is only talking about 'so-called' or 'alleged' forgetfulness. Because of an oversight (Timm: Letter to Hövelmann, dated June 20, 1983), these inverted commas were left out in the manuscript on which my response was based, and I wrote this response with the understanding that Timm was talking about 'genuine' forgetfulness. As I think the my counter-argument may nevertheless be of some interest to readers, I retain the relevant part of my response unchanged.
- 65 Ms. White has advanced similar arguments in her papers: "On the genesis of research hypotheses in parapsychology," in Parapsychology Review, 11 (1980), 1, 6-9; "Contribution to a roundtable on the future of parapsychology." Paper presented at the 25th Annual Convention of the Parapsychological Association, Cambridge, U.K., August 1982.
- 66 For details confer with Professor Zusne's comments on my paper as well as Zusne, L., "Contributions to the history of psychology: XXXII. On living with a specter: the story of anomalistic psychology," in Perceptual and Motor Skills, 55, (1982), 683-694. (I thank Professor Zusne for generously making a pre-publication copy of the galley available to me). Also cf. Zusne, L., "On conducting a zetetic dialogue," in Zetetic Scholar, 8 (1981), 118-122.
- 67 Zusne, L. (1982; in note 66), p. 685.
- 68 Ibid., p. 687.
- 69 Ibid., p. 691.
- 70 Zusne, L. Letter to the author, dated November 5, 1982.
- 71 Therefore, it is impossible for the moment to tell Richard Kammann "which side of Mishlove's fence" the parapsychologists "are sitting on." But I am sure that this dialogue has demonstrated that there are parapsychologists sitting on the side he would prefer. Cf. Kammann, R., "The parapsychologist at the choicepoint," in Zetetic Scholar, #8 (1981), 86-89; also cf. Mishlove, J., "The schism within in parapsychology," in Zetetic Scholar, 8 (1981), 78-85.
- 72 A much more detailed treatment of the problems to be discussed here is to be found in a paper on parapsychologists' reception of the Kuhnian historicistic views of scientific development and scientific change which I am presently preparing. I presented an abridged version of this paper -- under the title "Against historicism" -- at the 26th Annual Convention of the Parapsychological Association, Madison, N.J., August 1983, of which section III.1 of the present paper, again, is a considerably condensed version.
- 73 Kuhn, T.S.: The Structure of Scientific Revolutions. Chicago: University of Chicago Press, 1962.
- 74 For details, esp. cf. Lorenzen, P. (1969; in note 28) and Kamlah, W. & P. Lorenzen (1973; also in note 28).
- 75 There is one decisive methodological difference between physical and parapsychological experiments which I have tried to point out in my papers on the repeatability problem referred to in note 15. The difference between "experimental sciences" and "experimenting sciences" as well as that between "experimental actions" and "experimenting actions" is also explained there. These differences are irrelevant, however, as far as the purposes of our present examples are concerned.
- 76 For an explanation of what I mean by "factor psi," confer with my response to Professor Zusne.

# BOOK REVIEWS

The Enigma of Daniel Home: Medium or Fraud? By Trevor H. Hall. Buffalo, N.Y.: Prometheus Books, 1984. 160 pp. \$16.95.

Reviewed by Eric J. Dingwall

In his introduction to this book, the author states it is not intended to be a life of Daniel Home but is a collection of essays each of which tries to solve some puzzle connected with the medium hitherto not explained in any way that has been found convincing to himself.

The book is divided into ten essays together with an epilogue and introduction in which he lists a number of books and articles on Home dating from the middle of the nineteenth century. Among these mention is made of Mrs. Browning's famous letter to her sister in 1856 in which, discussing Home's visit to Italy the same year, she tells what she has heard of the medium's activity and of what she calls the "mystery of iniquity," the details of which were omitted in the printed version of the letter but which Dr. Hall was able to see in a copy of the complete letter, a discussion of which forms the subject of one of the essays where the author thinks that he has been able to resolve the matter.

The first essay asks the question as to who Daniel was and is a long disquisition on the origin of his second name Dunglas or Dungallass, a name not on the birth certificate but added later by Home clearly in order to connect himself with the Earls of Home, since he maintained that his father was a natural son of the tenth Earl of Home. This claim appears never to have been affirmed or denied by the Home family. Without citing any evidence, Dr. Hall dismisses it as an "outrageous lie" (p. 21) and as an example of Home's deceptions in regard to his origins. There then follows a long discussion of what Sir David Brewster wrote to his daughter (Mrs. Gordon) telling her what had been said at a meeting at which Home was present and whom he described as a lad of twenty, "the son of a brother of the late Earl of Home." Brewster does not say that it was Home himself who made this statement, and indeed Home later denied it in the second series of his Incidents in My Life (1872), saying that it was a matter of misinformation and that his father was the natural son of the tenth Earl.

Having made his point, the author tells his readers that Home's claim involved the tenth Earl in the "unusual pastime" of seducing one of the village girls, which seemed hardly likely in view of his age (64) and station in life. Thus he had become convinced that the medium's claims to an aristocratic connection existed only in his own imagination and in the minds of his gullible admirers.

The second essay concerns Home's early life in America and begins with a digression on the Fox family and the difficulty of determining the exact ages of Margaretta and Catherine at the time the rappings commenced. Quoting Podmore as saying that Margaretta was 15 and Catherine 12 at the time (1848), he omits to state Podmore's source, which is given as Capron who obtained his

information from Mrs. Fox herself, who stated that the younger girl was "about twelve" and the other was in her "fifteenth year."

Having discussed the ages of the Fox children, Dr. Hall proceeds to quote passages from Podmore's Modern Spiritualism which illustrate the general impression that Home made upon those who met him. These quotations and others in the book are indented in the text so the reader is clearly intended to accept them as being accurate copies from the original, but in many of them there are errors both in punctuation and in omissions which, although of little importance, suggest a lack of care in transcription which is to be regretted.

Finally, an account of Home's meeting with a number of well-known people is mentioned, among them being Mr. Rufus Elmer at whose house the medium had been staying. It was here that some very successful seances were held in the presence of some visitors from Harvard who drew up a report of what they had witnessed in terms very favorable towards Home but of which no mention is made by Hall.

In Chapter 3 the author discusses in greater detail than has hitherto been done what has been called "the mystery of iniquity" concerning an episode that occurred during Home's visit to Italy in 1855 and which was described by Mrs. Browning in a letter to her sister of March 4, 1856. As already mentioned, this passage was omitted in the printed version of the letter and the present reviewer obtained a photocopy of the original letter and contributed some notes on it to the Journal of the Society for Psychical Research for June 1970, p. 311, having previously mentioned it in 1947. Dr. Hall states that he has a copy of the omitted passage but does not say who was responsible for the transcription which contains a number of errors which are of little importance except from the point of view of accuracy.

According to Mrs. Browning, who got her information from Mr. Phipps, the brother of the Minister Plenipotentiary in Florence, a group decided to present Home with a great-coat and apparently arranged for him to buy one. He chose a very expensive one, keeping the money himself after arranging with the tailor to send the bill to those who had arranged the gift. In her Life of Home (1888), pp. 48-49, Mme. Home mentions this story saying that "it was quite untrue that Home had wronged Mr. Rymer in the matter of the coat but on the contrary had in 1859 sent a cheque for fifty pounds to Mrs. Rymer so that she could join her husband in Australia whither he had gone on account of financial losses he had sustained. This "pretty story," as Hall calls it, is marred by the unfortunate fact that Mrs. Browning's letter is dated 1856. The author has, however, confused two accounts, namely the one by Mrs. Browning and the later version by Mme. Home who was Home's second wife. She states that this story was many years later and went the rounds of the American press, although not quoting any extracts from the papers and the dates when they appeared. It does not seem to have occurred to the author that it was unlikely that Home would have told his second wife of the incident of the coat so that, when it was revived many years later, she confused the story with the gift to Mrs. Rymer. Following the chapter on the great-coat, the author proceeds to a discussion of the phenomena. He begins with an account of the early sittings at Cox's Hotel where Home was staying when he arrived from America and also at the house of Mr. J.S. Rymer, a solicitor who later, as we have seen, played a part in the "mystery of iniquity." In this chapter Dr. Hall makes a spirited attempt to show that some of the principal persons involved were more than

dubious about the genuine character of the phenomena they observed. The chief witness on whom Hall states he relies was Sir David Brewster, but his testimony on fraud on the part of Home is somewhat marred by what has been rightly called the "shameless disingenuousness" Brewster displayed in making contradictory statements which must have been known to Dr. Hall but which he chooses to omit. Fortunately, Brewster's contradictions and inconsistencies were exposed by those present with him at the relevant sittings, their testimony being later confirmed by his daughter (Mrs. Gordon) in a letter to her from her father written just after the sittings involved in the dispute. It is in this chapter that readers might expect to find Dr. Hall's own appraisal of Home's mediumship where he was able to answer the question "Medium or Fraud?" and if he considers the latter more likely in view of the fact that so many observers, mainly among the better educated classes, not only believed in the genuine character of the phenomena but never detected or exposed an undoubted example of fraud in spite of a few suspicions that had occasionally been aroused.

The reader will, I fear, not only be disappointed but often confused by Dr. Hall whose treatment of the subject in this chapter is founded on a number of speculations based on evidence derived from a variety of sources carefully selected for the purpose he has in mind. Thus he begins by quoting from the classic Davey experiments,<sup>2</sup> which illustrate the worthlessness of much of human testimony under certain conditions, and three examples are taken from the Davey papers. What he omits to tell the reader is that Davey revealed that he had employed a confederate to produce certain effects which had been observed and described by the sitters. Thus in these cases at least, Dr. Hall's theory that what the sitters saw was a total misinterpretation of the facts or that there were no phenomena at all but what was described was due to imagination of the observers falls to the ground. To strengthen his case, he quotes the present reviewer's comparison between some of the phenomena occurring with Frau Silbert and Mirabelli but makes no attempt to examine the three cases in detail and deal with the differences which, although not fatal to the comparison, make their value of lesser value. Thus in the case of Frau Silbert, few experienced persons have little doubt as to the normal character of most, if not all, of her effects but never, so far as I know, suggested that they were mesmerized or imagined everything while actually nothing took place at all as was, it seems, in the case of Mrs. Conway described by Dr. Hall or in what appears to have been a somewhat similar case Professor Harlow Gale described in the Proceedings of the Society for Psychical Research of Feb. 1900, pp. 65-899.

The most serious omissions in this chapter are due to the fact that Dr. Hall has made no attempt to quote or analyze any of the more striking examples of Home's mediumship such as the experiments with Crookes, the sittings in Holland<sup>3</sup> in 1858, and above all the accounts related by very many sitters as to the lighting conditions which in many cases but not in all make many of Dr. Hall's speculations untenable. Indeed, it may be said that this chapter contains not one example of striking manifestations reported by persons of competence and probity which were seemingly of such a kind that their paranormal nature had to be seriously considered. For example, in Italy in 1856 the Earl of Crawford, in a letter to his sister-in-law, described a sitting with Home at which the room was lighted by an oil lamp. After the sitting was over, those present including Home gathered round the tea table near the fire when suddenly a table with a loose marble slab on it at the further end of the room rose some three feet into the air. One of the sitters got up from the table at

which they were sitting and "with his utmost strength" tried to force it to its normal position but "had the greatest difficulty" in doing so.<sup>4</sup> Another example of objects moving at a distance was described by Professor Butlerov<sup>5</sup> at a sitting held in his house when Home was in Russia. He saw an arm chair move up to the table at which those present were seated and then "made a detour" to occupy a vacant space between Home and one of the sitters.

Another striking example of a remarkable incident was that when Mr. J.S. Bergheim described to Lord Rayleigh a meeting with Home. Bergheim, a noted mesmerist of the period, was a wealthy business man and was much interested in spiritualism, attending a number of seances. He was very friendly with Lord Rayleigh who had met him in the City and found that he had no use at all for what he called the "feeble tomfoolery" of the ordinary spiritualistic sittings, but that Home's performances "had no kinship" with them.

In about 1869 he told Rayleigh that he met Home one day in broad daylight with the sun shining into the window and began talking to him about levitation. He proposed an experiment which Home accepted and in which he placed his arms loosely around Home who then "floated up through their embrace, and landed on the floor."

In his general treatment of Home's phenomena. Hall is inclined to follow Podmore's guidance in the latter's views on the physical phenomena and quotes passages from his books where his views on Home are given. For example, he says that as Home was treated as a distinguished guest he was able to select his sitters and arrange their positions at the table. This may have occurred at times, but it was clearly not the rule since it was not usual to inform the medium who was going to be invited to the sitting. For instance, Mrs. Honywood, who knew Home well, said that she had often taken Home in her own carriage to the houses of her friends who were strangers to him and had there seen violent movements of furniture at sittings in rooms where she knew that Home had never entered until that moment.

Finally, towards the end of the chapter, the author discusses what he calls a damning ingredient in his critical assessment of Home. Indeed, he goes so far as to say that he considers it axiomatic that the honesty of a medium may be judged by his or her associates. What he refers to is a letter<sup>6</sup> to the eminent astronomer and solar physicist William Huggins (1824-1910) from Crookes in which he described a seance on April 11, 1871. The two mediums who gave the sitting were Charles Williams and Frank Herne, two professionals, the latter having a highly dubious reputation and whom Hall, rightly I think, describes as an unscrupulous trickster (p. 50) since both a few years later were exposed in blatant acts of deception.

It appears that Home had dined with Crookes on April 11 and was invited to accompany him to the seance which had been arranged for that evening. In the letter Crookes told Huggins that he had to induce Home to come since it was a dark seance and Home "always refused to sit in the dark" as he considered an absence of light unsatisfactory to those present. In this instance, however, he consented and Crookes told Huggins in enthusiastic terms about the extraordinary phenomena that took place.

Now, since, according to Hall (p. 50), if two mediums give demonstrations then both must be genuine or fraudulent, then it follows that as Herne was almost certainly a fraud then Home was one also. It would seem that Crookes

thought that both Herne and Williams may have been genuine, but what reason have we to suppose that Home, if genuine himself, knew that the two other mediums were fraudulent? That he wanted to see more is clear from the statement by Crookes in the letter that he agreed to attend another sitting on April 25. Whether this convinces the reader that Home's "demonstrations" show that he himself was a fraud is for him to decide, but there is no doubt that he later considered Herne to be an imposter since he refers to the exposure of him in 1875 which caused a sensation among believing spiritualists.

Chapters 5 and 6 consist of a rather tedious digression in order to decide various points regarding the date and other controversial issues concerning the privately printed book by Viscount Adare describing his Experiments in Spiritualism with Mr. D.D. Home and another in a discussion on the page-proofs. Dr. Hall considers that the date of issue of this book and certain alterations in the page-proofs show that some pressure had been put both on Adare and his father, Lord Dunraven, to press forward the appearance of the book in order that it might have some favorable influence in the coming legal battle with Mrs. Lyon.

For some reason that has yet to be explained, Dr. Hall describes this book both in the present volume and his earlier unpublished An Exercise in Bibliography and Textual Criticism (1971) as a substantial volume of over two hundred pages. Since his own detailed bibliographical description states correctly that existing original copies have only 180 pages, it is not clear what issue he has been using unless it be an odd mistake since, so far as I know, no copy of the original edition could be called "substantial" or had over 200 pages.

There is little doubt that Dr. Hall is quite right to suggest that Home did have considerable influence over Adare during the series of experiments described in this book, especially when he shared an apartment with him and was intimate with him and his friends in their everyday lives, and especially so in the later sittings such as No. 74 at Ashley House when it was obvious that Home had close relations with Adare since previous contacts with Bergheim were mentioned, and it seemed that Home had some fear of the strong magnetic influences emanating from that quarter. How far Home himself used his own powers of suggestion either consciously or unconsciously remains a matter of speculation, but, as Hall points out, they may have been exercised to a considerable degree at the famous window levitation which the author deals with in detail in Chapter 9 to which we will now turn our attention.

It was in 1965 that Dr. Hall first wrote a long account and analysis of this extraordinary incident which is too well-known to be described here. Suffice it to say that Hall again draws attention to the deplorable mass of contradictions and inconsistencies which abound in the accounts recorded by the witnesses which has led later students of the affair to come to a variety of conclusions as to the explanation of what actually occurred. Of these speculations those advanced by the author are some of the best, and much credit should be extended to him for the care he has taken in collecting the data and to account for them. He thinks (p. 126) that the mistakes made by the witnesses were too ridiculous and that there were too many of them, he suggests that those present must have been "in a mildly abnormal state" and that Home was "one of those rare individuals" who possessed the power of imposing suggestions upon others to a marked degree. In any event, he is of the opinion that Home strode quite normally over the window sill and on to the balcony and

thence to the adjoining balcony and window. Thus the case suggests that the alleged levitation was simply an ingenious fraud perpetrated by the medium for his own advantage in a crisis in his own career.

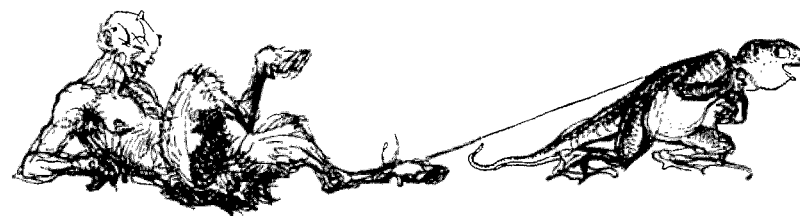
If this view be accepted, and it does, I think, deserve serious consideration, then the question in the title of this book Medium or Fraud? would have to be answered by a statement such as Medium or Fraud: or Both? and this view would be one applicable to many others in the frustrating and tangled history of psychical research.

In Chapter 10, Dr. Hall sums up the conclusions at which he has arrived after compiling this rather imperfect but provoking book. He advances the view that "Home's principal secret lay in his peculiar ability to influence his sitters and those with whom he came into contact" (p. 139). As he does not attempt to define the words "peculiar" or "abilities," it is impossible to determine what meaning he wishes to attach to these words. By "peculiar" does he mean odd or something exclusive to an individual? And in what sense are we to understand his use of the word "abilities"? Taken by themselves, the words do not appear to throw much light on what theory Hall favors to decide whether Home was a medium or a fraud.

The chief lesson to be learnt from this book is that the enigma of D.D. Home remains an enigma, and there is no sign of it being resolved.

#### NOTES

- 1 D.D. Home: His Life and Mission. By Mme. Dunglas Home. London: Trübner & Co., 1888.
- 2 Proceedings of the Society for Psychical Research, IV, 1887, pp. 381ff.
- 3 D.D. Home, il Medium. By George Zorab. Milano: Armenia Editore, 1976.
- 4 For an account of this unpublished letter, see The British Journal of Psychology (General Section), Pt., Feb. 1953, pp. 62ff.
- 5 Quoted by Thomas Berry from Ruskii Vestnik and published in the Journal of Religion and Psychical Research, vol. 7, no. 4, Oct. 1984, p. 224.
- 6 The Life of Sir William Crookes, O.M., F.R.S. By E.E. Fournier D'Albe. London: T. Fisher Unwin Ltd., 1923, pp. 191-193.



The Truth About Astrology [U.K. title] or Birthingtimes: A Scientific Investigation of the Secrets of Astrology [U.S. title]. By Michel Gauquelin. Oxford: Blackwell, 1983; New York: Hill and Wang, 1983. 204+ix pp. \$12.95 hardbound; \$7.95 paperback (1984).

Reviewed by Piet Hein Hoebens

The range of topics discussed in Michel Gauquelin's latest book is somewhat narrower than the British title suggests. The Truth About Astrology does not offer a comprehensive evaluation of astrological claims. Apart from the chapter "The Horoscope Falls Down" (debunking traditional astrology), the book deals almost exclusively with Gauquelin's own work in "neo-astrology."

Readers familiar with Gauquelin's earlier publications will find comparatively little new information in The Truth. The book recommends itself as an excellent summary of "neo-astrology" and is particularly interesting for what it reveals about its author's intellectual personality.

In the concluding chapter, Gauquelin confesses: "Though I am so full of my subject, so determined to defend it, so proud of my discoveries, I am still tormented by two feuding demons. The first is the fear of having been mistaken in asserting that astral influence is real; the second is the agonizing thought of all I have been unable to discover or explain."

Gauquelin describes his own relationship with astrology in terms of "passion." His passion, however, is a complex affair. At the same time he is repulsed by astrology's inanities and attracted by its quaint and mysterious charms.

A case in point is his attitude towards the astrological tradition as an "explanatory model." Methodologically a conservative, he assumes -- most of the time -- that the "planetary effects" he believes he has discovered will eventually be explained in terms of non-occult physics. However, occasionally he wonders whether "perhaps I am making a mistake in trying to rid the planetary effect of all 'absurdity'" and "to substitute a rational and convincing argument for the astrologers' explanations" (p. 159).

Obviously, Gauquelin finds it hard to make up his mind. Here I cannot but sympathize with him. Much as I am impressed with the quality of the evidence, I must confess to being unable to make any sense of these planetary effects. All attempts to explain them (i.e., to suggest a way they might cohere with the rest of nature) strike me as in varying degrees implausible.

Although I agree with Professor Abell that more independent replications will be needed before all doubts about the rectitude of Gauquelin's data have been dissolved, I notice that Gauquelin's claims have proven resistant to debunking attempts to such a degree that, in this exceptional case, I would personally place my bets on the proponent rather than upon the critics. However, the question "What does it all mean?" remains an open one.

A major part of The Truth is concerned with the search for a rational explanation of the strange findings, i.e., an explanation that assumes the reliability of the data and tries to account for these without appealing to occult forces or influences. Gauquelin's favorite guess is that the effects may be

correlated with changes in geomagnetic activity caused by the Moon, Mars, Venus, Jupiter and Saturn. The foetus is presumed to be able to detect minute variations in the magnetic field and to choose to leave the maternal womb when the planet most congenial to its "genetic temperament" is rising or culminating on the horizon.

One of the major problems with this proto-theory is that it does not adequately explain why the actual distance between the Earth and the "midwife planets" does not seem to make any difference for the strength of the effects. Neither does it explain why the effects are noticed only when the planets find themselves in two of the twelve celestial sectors.

Gauquelin is very well aware of the "distance" obstacle (p. 152) but almost seems to have forgotten it when, in the very next section, he discusses the magnetic field hypothesis. To complicate matters even further, he suggests on p. 158 that the enormous distances might account for the apparent non-existence of any Uranus, Neptune or Pluto effects.

It is not clear to me to what extent Gauquelin's "naturalistic" research programme may be expected to lead to a solution of the puzzle. To the philosophers and sociologists of science, it will be interesting to watch what Gauquelin will do if and when he comes to the conclusion that his search has failed. Will he acquiesce in the conclusion that he has discovered one more Fortean phenomenon, an inexplicable oddity in nature? Or will he finally surrender to the siren song of astrological occultism?

Traditional astrology does not explain anything, but it did to a certain extent anticipate Gauquelin's positive findings (although it is flatly contradicted by Gauquelin's even more numerous negative findings), and at least it provides a terminology eminently suited for concealing our ignorance. Even more important, it intuitively appeals to those who, for one reason or another, have become disenchanted with the world view of mainline science.

Michel Gauquelin is both a skeptical inquirer and a proponent of a Claim of the Paranormal. I very much wonder how he will eventually solve the identity problem resulting from this strange situation. The Truth About Astrology gives no unambiguous answer. Gauquelin is still too confused about his own discoveries.

Readers of Zetetic Scholar will be particularly interested in Gauquelin's account of his confrontation with the scientific establishments in France, Belgium and the U.S. Perhaps it is a pity that the relevant chapter, "Science and Proof" was written at a moment when the chances of an honorable peace to end the Mars Effect war with the Committee for the Scientific Investigation of Claims of the Paranormal seemed close to zero. The remarkable "Reappraisal" by Professors Abell, Kurtz and Zelen in The Skeptical Inquirer (which Gauquelin acknowledges in a note added at proof stage) has since resuscitated hopes of a happy ending. I understand that Michel Gauquelin's current view of CSICOP is far milder than the casual reader of The Truth would probably expect.

A few critical remarks: I found the penultimate chapter "'Neo-astrology' Under Attack" not entirely convincing. Gauquelin laments modern obstetric policies in the West ((with Holland, I am happy to say, as the favorable exception) because "the mechanisation of childbirth" may affect "the link binding us to the cosmos and the evolution of our species" ((p. 175). It may be true that



modern obstetric techniques destroy the neo-astrological effects, but to add in this context the warning that "to violate the laws of nature may have serious unknown consequences for the future of our descendants" is a little insipid. Theologians have used this argument against every technological innovation from the invention of the wheel to the introduction of contraceptive devices.

I do not understand why Gauquelin takes for granted (p. 178) that the observation of planetary effects at birth would be a scientific revolution of Copernican magnitude. Until some sort of "explanation" is found for the neo-astrological anomalies, it will remain futile to speculate about their implications for science. An explanation in terms of "disturbances in the magnetic field" (Gauquelin's favorite), remarkable as it might be, would probably leave the temple of orthodox science pretty much intact.

Apart from these quibbles, I found The Truth About Astrology an excellent book by one of the most interesting and engaging figures in modern anomalistics.

Reviewed by Geoffrey Dean

Michel Gauquelin has been tirelessly researching astrology since 1949. This book tells the story of his work and is essential reading for anyone seeking an up-to-date overview. It is also the story of determination triumphing over immense tedium, mindless hostility, and lack of resources. For those who see research only in terms of dollars this book will be a revelation and an inspiration.

It is aimed at the general reader. As usual with a Gauquelin book, it is very clearly written. It has 29 handsome figures (mostly graphs), 7 tables, 200 references, an index, and a questionnaire asking for birth data and a character assessment (via a check list) to help further research. The topics include occupation, heredity, character traits, unsuccessful attempts to demonstrate character effects with ordinary people, the CSICOP affair, a look at various astrological claims including signs, possible mechanisms for the planetary effect and criticisms thereof, and a survey of modern obstetrical practices and their relation to planetary effects. The last two topics will be of especial interest to anyone trying to formulate an explanation.

It is only a few years since Michel Gauquelin's two previous books in English on his work were published, namely Spheres of Destiny (1981) and Cosmic Influences on Human Behaviour (1976). Hence much of the information is not new, but this is the price for having an up-to-date account in one volume. Similarly many technical points of interest have had to be left out, for example the problem of expectancy, but the source material is amply documented for readers wanting more details.

Three things shine through: Gauquelin's enthusiasm for his subject, his scrupulously scientific approach, and the sheer mass of evidence accumulated to date. Readers who are aware of Gauquelin's work only through the writings of his critics will know that his results are supposedly all due to selection of data, wrong use of statistics, and lack of replication, if not to falsification of data and sheer pig-headedness. The facts documented in this book tell quite a different story.

Right from the start Gauquelin's approach has been a model of the scientific method, with replication and publication of data every inch of the way. Thus in 1951, when he observed planetary effects for 576 famous French physicians, his response was to perform a replication with another 508 famous French physicians. The results were the same. So he tested other French professionals. The results were again the same. In 1955 he published all the results and all the data (5756 cases), and extended the tests to other countries and to heredity. Thus from the very beginning his work has continued for the very best of reasons -- because the results replicated.

Because this early work was published in French, few people are aware of how rigorous it was. To be sure, it is briefly mentioned in Gauquelin's later English books including the present book, but it seems to have been conveniently forgotten by his critics. Today, some 100,000 cases later, the total evidence (as opposed to the tiny part involved in the Mars Effect) is quite massive and consistent across occupation, character and heredity. There is a diurnal effect for the moon, Venus, Mars, Jupiter and Saturn, but not for the sun or the other planets. There is no zodiac effect for anything. And according to work too recent to be included, there is no aspect effect either.

The diurnal effect has many baffling features. It disappears if the person is less than eminent, and if the birth is induced or surgically assisted, and is enhanced if geomagnetic activity is high at birth or if both parents have the same planet emphasized. So it seems to be a genuine physical effect rather than an artifact of psychology or demography. One explanation may be that the fetus tends to trigger its own birth only if the planetary signals are appropriate, but as Gauquelin points out even this poses baffling problems. Thus none of the obvious signals -- electromagnetic radiation, magnetism, gravity -- fit what we know about the planets and their solar-terrestrial effects. If the planet helps to trigger labour, why is the effect observed at birth? Why birth, when character was presumably formed months before? And what possible natural advantage could a planetary effect bestow? One suspects that there is a simple explanation, but to date nobody has the slightest idea what it might be.

The standby explanation in such cases, namely self-fulfilling prophecy or believing is seeing, seems unlikely to apply to an area as unfakeable as eminence, especially as most people are unaware of their planetary positions in the first place. Self-fulfilling prophecy would be more likely if there were closet astrologers among the parents of eminent people or among their biographers -- except

that the prophecies to be fulfilled tend to contradict astrology, which predicts weakness rather than strength for the positions observed. A necessary first step towards solving the mystery would seem to be a careful review of the many studies of eminent people for clues relevant to astrology, and a factor analysis of the Gauquelin trait word pool, but such ploys have so far escaped attention.

To be sure, there is a need for more checks and replications of Gauquelin's work by others, even though as Eysenck pointed out nearly ten years ago "as far as objectivity of observation, statistical significance of differences, verification of the hypothesis, and replicability are concerned, there are few sets of data in psychology that could compete with these observations" (New Behaviour, 29, May 1975, 246-249). Fortunately such checks need not be difficult. After all, the complete Gauquelin data are available on magnetic tape and can be readily checked against the source data in the public domain and on the original birth certificates accessible at Gauquelin's laboratory. The few independent checks that have been done, and Gauquelin's own check of his original hand calculations using the computer of Astro Computing Services in San Diego, have found no appreciable errors (see Correlation, 1984, Vol 4, No 1, in press).

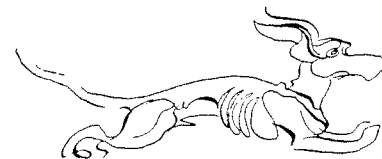
I personally visited Gauquelin's laboratory in Paris for a couple of days in 1981 and again in 1983, and was most impressed by the excellence and organisation of his records (which fill drawers occupying an entire wall), the extent of his and Francoise Gauquelin's writings on cosmic influences (their various books in half a dozen languages occupy several shelf feet), and the extent of their writings in orthodox areas such as psychology (their various books in French occupy a comparable space). I was also impressed by something that to my knowledge has never been mentioned in print: unlike many scientists I have met who seem to have a brick wall between the ears when it comes to ideas other than their own, Gauquelin is very easy to talk to. In fact having at various times been invited to criticise his draft articles, and having once in a parallel study obtained exactly opposite results to his (we refereed each other's study which then appeared side by side in Correlation, 1981, Vol 1, No 2), I would put Gauquelin's open-mindedness, his willingness to act on comments, and his willingness to be proven wrong, as second to none. Which makes the behaviour of some of his ostensibly scientific opponents (some of which he describes in the book) all the more distressing. If nothing else Gauquelin's work has generated interesting case studies for historians of science. One looks forward to his autobiography.

Since readers who are skeptical of Gauquelin's work will now disbelieve everything I have said, let me finish by looking at the comments of other reviewers.

First, reviews by astrologers. Whatever you may think of astrologers, if you are investigating astrology then their views are needed to ensure that you are not setting up straw men. To date I have found a total of six reviews in serious astrology journals in the UK, USA, Australia and New Zealand. With one exception the reviews are most favourable and say things like "eminently readable," "a very human document... of absorbing interest," "contains a great deal of interest and concern to a majority of astrologers," and "the best single source of Gauquelin's work for the lay reader." The exception dismisses his work on the grounds that the statistical approach cannot be used to test astrology because it "tends to assume that everyone responds equally to planetary impulses." Thus "a strong Aries content... does not mean necessarily that the native will manifest the characteristics of Aries freely and naturally." The reviewer evidently does not recognise the implications of non-falsifiability. One review noted that the "graph axes are either labelled insufficiently or not at all," but I couldn't find any graphs requiring labelled axes that did not have them.

Now to reviews by non-astrologers. Here I have only three, namely one by Piet Hein Hobens elsewhere in this journal, one by Dr Michael Startup in Correlation (1983, Vol 3, No 2), and an anonymous review in Astro-Psychological Problems (1984, Vol 2, No 3) that expresses no opinions. The first two reviews are both favourable but point out that the book does not deliver what the title of its UK edition promises. That is, it says a lot about Gauquelin's research but very little about anybody else's research, so anyone seeking the full picture is far better served by a copy of Eysenck and Nias's Astrology Science or Superstition, now available in Penguin paperback, and by the forthcoming revised paperback edition of Culver and Ianna's Gemini Syndrome. Gauquelin tells me that this is the fault of the UK publisher, who defined the topic and then insisted on a misleading title. He also tells me that the first translation of his French manuscript was so poor that it had to be redone by a second translator, which left him insufficient time to check it properly before the deadline. No important errors were found.

In summary, as an up-to-date and highly readable account of Gauquelin's work in one handy volume, this book has been unanimously recommended by its reviewers. Nobody who is about to express an opinion on Gauquelin's work should open his mouth without first consulting it.



Anomalistic Psychology: A Study of Extraordinary Phenomena of Behavior and Experience. By Leonard Zusne and Warren H. Jones. Hillsdale, N.J.: Lawrence Erlbaum Associates (Wiley), 1982. 498 pp. \$29.95.

Reviewed by Ron Westrum

This lengthy and useful book by Zusne and Jones is intended as an undergraduate text for "psychology of the paranormal" courses, and it serves this purpose well, if incompletely (see below). It covers many topics relevant to most undergraduates' concerns with the kind of issues dealt with in ZS. It references a large body of literature that has been for the most part well-digested by the authors. I learned a considerable amount about the areas of anomalistics that were not familiar to me, and I trust this will be the case with most of the book's readers. Its coverage includes ESP, psychical healing, astrology, spiritualism, UFOs, dowsing, poltergeists, and reincarnation, to name only a few of its subjects. I recommend it to the reader beginning study of this area, however, with certain reservations.

These reservations have to do with the methodological and theoretical adequacy of the book. The book's problems are not so much what is in it, but what it leaves out. There are three major problems in the book's treatment. These are: 1) an inadequate definition of its subject-matter, the extraordinary; 2) lack of treatment of the psychology of genuinely anomalous perceptual stimuli; and 3) little treatment of the sociology of crypto-scientifically anomalous events. These problems are inter-related, and are reflected in occasionally biased descriptions of proponents and opponents of the paranormal. For instance on page 294, they refer to Hansel's "devastating but irrefutable critique of ESP." Devastating, perhaps; but irrefutable? Surely we have moved from the world of science to world of casuistry here. On p. 43, they suggest that one reason that John Fuller is unreliable is that he is "an author, not a medical expert, who (4) also is a flying-saucer enthusiast who contributed two best-selling sensational books on flying saucers in the 1960's." Suppose the sentence had said that Philip J. Klass is "an author, not an astronomical expert, who also is a professional flying saucer debunker who has produced three books poo-pooing the subject." This latter statement is correct, but we would all recognize it as slanted. Yet the statement about Fuller is surely no less slanted. Actually, having investigated UFOs for some time than, I find the two books by Fuller to be more factual and straight-forward than those by Klass; they are sensational because their subject-matter is sensational. In their chapter treating UFOs, in fact, Zusne and Jones routinely reference popular science writers such as Klass, Oberg, and Sheaffer, and hardly refer to scientific experts such as Hynek. Yet they do not point out that, e.g., "Oberg is an author, not an astronomer." Their treatment of UFOs shows more bias than most of the other chapters, I suspect, because the authors have read much less of the literature in this area, and depended largely on CSICOP sources. But let us consider some of the more fundamental problems.

1) The Problem of Definition. What is the extraordinary? To this thorny problem, about which so much ink has been spilled in ZS, the authors take a surprisingly casual approach. Following Broadbent, they

define the anomalistic as those experiences whose stimuli seem to fall outside certain "basic limiting principles." Regarding the latter, they give the following examples:

1. An effect cannot precede its cause;
2. No one can literally read another person's mind;
3. Objects cannot be moved simply by willing them to move;
4. Nor can objects be transformed into other objects by willing them to do so;
5. A person cannot be and act in two places at the same time.

How "basic" some of these principles are is open to dispute, to say nothing of their ambiguity. What precisely, for instance, is meant by #2? Does this mean that it is impossible to know what another person is thinking? Under what conditions? What does "read" mean? Furthermore, in what sense do astrology and UFOs violate any of these? The actual definition of "anomalistic" employed by Zusne and Jones is not "violation of basic limiting principles," but a much broader one such as "inconsistent with current scientific doctrines." It should be immediately obvious that everything that falls into this latter category is not to be immediately discarded.

To give merely one example, in discussing UFOs the authors make it seem unlikely that UFOs could come from another star system, since interstellar flight is very difficult (for us humans). The authors are apparently unaware that even as their book was finished in 1979, there was a convention of astronomers who felt that interstellar travel was certain for a species of advanced intelligence. Since there was no evidence(!) of extraterrestrials on earth, this proved that there were no advanced intelligences, since such intelligences would certainly have come here if they existed. Thus UFOs, if real, would not violate any limiting principles, but would simply seem implausible according to the (then) dominant majority in astronomy which felt that such intelligences existed but were limited to radio signals for interaction with us.

It is obvious in considering this example and others in the book that what the authors really mean by "basic limiting principles" is essentially "current common sense in science." For the authors must know that what science has considered "basic limiting principles" has changed over the last few centuries. Thus what is anomalous today is what is currently unacceptable to science. Thus they feel that when someone comes to believe in or experience an anomalous event (thus defined), there must be something wrong with him or with this experience. The book is a detailed catalogue of how perception and belief (not behavior, as their title would imply) can go wrong. That such perceptions can be correct, as they have been from time to time in history, is never considered by the authors, although we, as scholars, must consider it.

2) The Psychology of Genuine Anomalistic Stimuli. One subject hardly touched on by the authors is what happens when a genuinely anomalous stimulus is perceived. This is most disappointing, since the same defect is embodied in Graham Reed's The Psychology of Anomalous Experience. This subject is hardly an unfamiliar one, for the authors must surely know of Hebb's experiments on fear and Bruner's experiments with the creation of "impossible" stimuli such as red aces-of-spades.

As a sociologist who has done a considerable amount of field-work involving stimuli which have stimulated such reactions, I have been forced to develop my own psychology of anomalous experience, although this work has yet to find complete expression in any of my writings. I plan to address this subject in a book under way, to be called something like Anomaly and Society.

For instance, there is the variety of reactions to apparent anomalies which appear in such TV programs as "Candid Camera," and whose careful study would be extremely valuable. I myself have thought about, but not carried out, a clinical study of fear using well-reported UFO cases. I will never forget the degree of upset produced in a party guest when an amateur psychic told her (they had not previously conversed) her mother's name. The surprise, fear, and even trauma of percipients of anomalous events need discussion. The authors discuss cognition and motivation, but they seldom deal with the post-event sequelae which I, as a UFO investigator, know only too well. I first encountered the post-traumatic stress syndrome when studying UFO close-encounter witnesses.

What of the social psychology of the anomaly witness in relation to the family, friends, official agencies, the media? The authors say nothing about there. They explain how bogus anomalistic experiences can be stimulated through group interaction. They say little about how the group can act to suppress the genuine experience of, e.g., a ball lightning or bolide witness. Again and again, I have come across persons who have had anomalistic experiences but kept them secret because of fear of social rejection. I would suggest to the authors the value of their making a study of the interactions of pilots and official agencies regarding ostensible UFO sightings, as a case-study in the working of norms to suppress the anomalous.

Then there is the psychology of the scientific community regarding anomalies, which I have addressed in a series of articles, partially summarized in Knowledge Vol. 3 #3 (1982). Although the authors seem knowledgeable about the history of psychology, their knowledge of relevant episodes in the history of science is not evident in the book. They mention N-rays but not meteorites. Some "extraordinary phenomena of behavior and experience" took place when J.L.B. Smith announced that he had obtained the carcass of the Coelacanth (see my essay in ZS #10). Students---the audience for the Zusne-Jones book---need to know not only the sociology of UFO crazies but also the sociology of the scientific community. While on page 97 the authors indicate that there are fashions in psychology, they need to point out that there are fashions in the hard sciences, too. Science in this book is usually seen as the alternative to the foolishness and knavery is discusses. It needs to be stressed that science, too has social processes. The recent anthology by Harry Collins, The Sociology of Scientific Knowledge: A Source-book, might be a useful supplement to the Zusne and Jones book, at least for advanced students.

3) The Sociology of Anomalies. Most of the book is devoted to what Truzzi calls the parasciences, those kinds of anomalies which consist of a relationship between two orders of otherwise unremarkable events:

being born and celestial positions, talking and healing, premonitions and subsequent dramatic events, etc. But many anomalous events do not involve this kind of inferential basis. They deal instead with solid objects (UFOs included) whose existence could be proved by production of a specimen. Typically also, there is no way that such phenomena can be experimentally produced (unlike ESP, psychic healing, dowsing, etc.). Bigfoot, lake monsters, earthquake lights, the Congo dinosaur, UFOs, the fall of unusual objects from the skies---these are different from most of the phenomena discussed in this book in some ways, but not in others. These differences are worth comment. For instance, potentially one can photograph crypto-objects. The photographs can be analyzed. They may leave traces, which can be analyzed by scientific specialists. If the obvious way to assess alleged para-events is to set up a laboratory experiment, the obvious way to study crypto events is to mount a field expedition. According to the authors, "modern parapsychologists have excluded spontaneous case materials as scientific data" (p. 281), due to fallible memory, in regard to apparitions. If so, it is interesting to note that most articles on ball lightning by physicists use largely eyewitness data. The correct use of eyewitness data is an important subject which the authors scarcely address, but is an important one for crypto events. Useful indications will be found in Arne Trankell. The Reliability of Evidence, and in several papers by H.H. Nininger on meteor observations, including his paper in ZS #10.

In this regard it is useful to mention the changing perception on the part of the scientific community of hissing and buzzing noises heard simultaneously with high-altitude meteor flight (see Corliss, Earthquakes, Tides, Unidentified Sounds and Related Phenomena, 1983, pp. 165-169). Previously these noises were treated a la Zusne and Jones, but the accumulation of cases has gradually shifted attitudes so that mechanisms for the production of these anomalous (faster than sound) noises are now being sought. I would strongly recommend this case study (Corliss gives 35 references) as a supplement to the Zusne-Jones book. It shows how the assumption of a psychological basis for a phenomenon may impede its physical investigation.

Finally, there is the question of the existing anomaly literature, which Zusne and Jones deal with in passing (they show a good knowledge of the history of psychical research), but which needs a more detailed treatment than they are willing to give it. There is the review by Truzzi and myself in ZS #2, but this is only a beginning. Although there are many popular treatments of the literature, there is (to the best of my knowledge) nothing general that is serious. Ironically, there is a large scholarly literature on the "Wow" writings of ancient times and the Renaissance but little on that of the present. There is the book Superstition and the Press by Curtis MacDougall and Georges Auclair's Le Mana Quotidien, but these deal only with ephemeral publications. More is needed.

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I have dealt here largely with the defects of Anomalistic Psychology, but this is out of necessity. If this book is to be used widely with undergraduates, it is essential that they---and their teachers---understand that this is not the whole story. I have tried to present

here some of the book's problems and how they might be remedied. Before this book can be endorsed for classroom use, appropriate supplemental materials need to be designated. It is to be hoped that this book will spark a continuing discussion on how a truly adequate psychology of anomalistics can be developed. In this regard I see myself on the same side as the authors although my perspective is quite different from theirs. I am sure my own book on the subject would have opposite defects from theirs. All the more reason that we should regard Anomalistic Psychology not as a definitive summation, but as a step toward a broader and deeper knowledge of this subject-area.



Psychic Warfare: Threat or Illusion? By Martin Ebon. New York: McGraw-Hill, 1983. 282 pp. Cloth \$15.92.

Reviewed by John Beloff

Once you are prepared to admit the reality of psi phenomena, as I am inclined to do, you can never exclude the possibility that they might eventually be exploited in all sorts of fantastic ways both nefarious and beneficial. True enough, my own long experience tells me that we still understand so little about such phenomena and have gained so little control over them that even the most modest applications that one could envisage are still a thing of the future. Yet, at the same time, one must acknowledge that the research effort that has so far gone into this field has been minimal. I doubt whether all the man-hours that have ever been spent on serious psi research would exceed that which, say, NASA would require in a month. Suppose, therefore, that somewhere behind closed doors a massive research program were now in progress. Is it not conceivable that startling results might already have been achieved, the psychic equivalent of the Manhattan Project that gave us the original atom bomb? This is not, I confess, a scenario that I find at all plausible but it has, perhaps, just sufficient verisimilitude to tease us.

The most sensational thing about the present book, I would say, is its title. The contents consist largely of a sober enough survey of parapsychological research in the Soviet Union, or, rather, what can be ascertained about it, since around 1962 when Professor L. L. Vasiliev published his Experiments in Distant Influence in which he describes experiments he had undertaken during the 1930s when work of such a kind could not be published. Ironically, as Ebon points out, it was a fictitious report in a French popular science journal, Science et Vie, that brought about the revival of parapsychology in post-war Russia for, according to this report, the US Navy had been conducting telepathic experiments with the crew of the Nautilus (the first atomic-powered submarine) when the ship was cruising under the north polar ice-cap in 1958. From that time on the party line on parapsychology alternated between spurts of official encouragement and the occasional official clamp-down. Perhaps the closest that the Soviet authorities ever got to formulating a policy on parapsychology was in 1973 when an article appeared in the prestigious journal Questions in Philosophy (which is here reproduced in full in an appendix) signed by four academicians including Leontiev and Luria, both well known in the West. The gist of their recommendations was that parapsychology was too important to be left to the parapsychologists and should henceforth be taken over by respectable scientists like themselves.

My own impression of Soviet parapsychology, for what it is worth, which reading my Ebon has done nothing to dispel, is that it lags sadly behind its counterpart in the West. It has brought to light some spectacular subjects, like Nina Kulagina or Rosa Kuleshova, who have earned a chapter each in this volume, it has also had its martyrs, like

the unfortunate but indomitable Edouard Naumov who rates another chapter, but what is lacking is that continuous critical tradition that is such a feature of the Western scene. There is no parapsychology journal, even, in the Soviet Union which might have been able to set certain standards. One consequence of this is that it has become encrusted with pseudoscientific notions appropriated from physics and, although this has not prevented it from impressing certain Californian research groups whose advice was sought by the CIA, it is an aspect that is viewed with suspicion by the leading parapsychologists in the West. In the circumstances, it is greatly to the credit of these Soviet pioneers that they have achieved as much as they have done. Ebon pays a fitting tribute to their courage when he declares (p.204): "I feel strongly that they have not done better than experimenters in the West - but they have tried harder, with fewer inhibitions, with gusto, imagination and persistence."

Such, at least, is the impression we get. But what if this impression is false? What if Soviet parapsychology, as we know it, is just a smoke-screen? Part of an elaborate disinformation operation? There is no doubt that it is widely believed in the West that the Soviet government has been spending vast sums on psi research. As recently as 1981, the Committee on Science and Technology of the US House of Representatives in a report they issued lent support to this view. Ebon even cites a figure of \$500M as an estimate of Soviet expenditure per annum on parapsychology although he does not say how the figure was arrived at. Yet, if anything like this is correct, what is happening to all this investment? Is there any evidence that the Soviet rulers are getting value for money? Ebon cannot tell us. What he does say is that in recent years there are many indications that psi research has come increasingly under the direction of the KGB. No one is better qualified to judge, since Ebon is a recognized authority on Soviet affairs no less than on parapsychology, yet the indications he mentions strike me as tenuous and oblique. There is a cynical interpretation of these developments which is that the Russians are pretending to be involved in such research in order that the Americans might be tempted to waste their own defence spending in this way but that is perhaps somewhat too far fetched.

Whatever the explanation of Soviet activity in this field, is there any evidence that the Pentagon is rising to the bait? An article by William J. Broad in the *New York Times* (Jan 10) discusses this question and mentions the various rumours that seem to point in this direction. Ebon who devotes one chapter to 'The Washington Dilemma' does likewise but is equally non-committal. For example, a memorandum recently released under the Freedom of Information Act reveals that the CIA were already beginning to take an interest in parapsychology back in 1952 although, so far as one can tell, nothing much ever came of it. Ebon suggests various reasons why the US government agencies might want to operate under cover including plain fear of ridicule, but he points out how difficult it would be in America to maintain secrecy seeing that "the number of parapsychologists is small and they are always talking about each other's

work."

Can we say, then, that talk about 'psychic warfare' is, to say the least, premature? Is it still in the realm of illusion rather than threat? That, at any rate, would be my conclusion after reading this conscientious attempt to set out the facts of the case insofar as these are known. We may still have good reason to fear the Russians, but their mastery of the secrets of the paranormal does not appear to be one of them.

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*Superstition and the Press.* By Curtis D. MacDougall. Prometheus Books, New York, 1983. xi+616pp.

Reviewed by Henry H. Bauer

According to the author, "This book documents what anyone whose only source of information is newspapers would know about contemporary superstitions." MacDougall treats us to more than 600 pages on each of which there is reference to perhaps half-a-dozen items from newspapers about subjects which the author labels superstition. That amounts to a huge and rich resource for anyone who is interested in one or more of these topics, which others might call paranormal or anomalous. The news items themselves are useful raw material in several respects (but not usually about the facts of the given anomaly: MacDougall makes plain throughout how substantively misleading coverage in newspapers typically is); and one can learn important things also about what newspapers do, and how. MacDougall makes the following points and illustrates them:

1. Newspapers commonly follow up on their major stories and features -- except when the topic falls within the class of anomalies. There the reader is typically left hanging: having read that some marvelously strange creature has been found, the reader of newspapers is not thereafter told whether the animal was ever captured, or identified, or unmasked.
2. The media are easily hoaxed, and hoaxes have often been widely published, quite naively.
3. The newspapers do not take a consistent stance on these off-beat subjects: a given paper may be derisive about a given topic in July only to be quite straightfaced about it in August, and jocular again in September.

So there is much of interest in this book, and indeed students of anomalies must become acquainted with it: sooner or later, each can profit from delving into particular sections of it. Unfortunately, I must draw attention also to some unsatisfactory features of the book, which caused me to become greatly disappointed by it.

Such a book could be simply a compendium without analysis; or it might aim to make some such points as the three offered above, using the newspaper items as illustrations only. This book falls somewhere in

between, and does so inconsistently. In some chapters, page after page consists of a sentence or two simply describing what newspapers have said about a particular topic; but other chapters contain analytical and critical commentary by the author as well as descriptions of stories. The straight reportage is exceedingly useful source material, but it does become very tedious if one attempts to read these sections at a rate of more than a few pages at a time. Where MacDougall becomes analytical, the book also becomes more interesting reading -- but also less reliable: MacDougall has strong opinions indeed and seems to suffer from no suspicion that his opinions might sometimes be incorrect; moreover, he claims (page vii) that what is said in the book was learned from newspapers and not from magazines or books -- if that is the case, then any analytical comments must be unreliable indeed! At any rate, I have no doubt that readers who are knowledgeable in a particular field will find MacDougall in error on an appreciable number of occasions. For example (page x), he says that "Starbaby" by Dennis Rawlins is "in support of the so-called Mars effect", which is surely a significantly misleading characterization of that piece.

Since I know more about Loch Ness than about the other chapters in the book, I shall draw my further examples from that section. It is a serious error to claim (page 283) that the tale of St. Columba was not included in any reports from Loch Ness until the mid-1960s. "The uninterrupted series of failures to obtain any scientific evidence" (page 284) is rather too dismissive of Dinsdale's film of 1960, the underwater photographs of 1972 and 1975, and the sonar results obtained by several different teams over a couple of decades. MacDougall's reasoning or chronology is faulty when he says (page 285), "what caused the press to cease its expressions of disrespect was the multiplicity of sightings dating from the construction in 1933 of a highway along... Loch Ness": this comes after he has just quoted disrespectful expressions dated between 1947 and 1967! And in any case, there were almost no mentions of Loch Ness, respectful or otherwise, before 1933. More errors: the Surgeon's photo is not (page 285) the only one resembling a monster; and it has been compared to an elephant's trunk, not to an elephant's tail. There is confusing chronology again (page 288) when the hippo-spoor hoax of 1933-34 is mentioned in the same place as the books of Whyte (1957) and Dinsdale (1961). The latter's film was taken in 1960, not 1959; and the Royal Air Force Report did not estimate an actual length of "nearly 100 feet" (page 290).

Some things that annoyed me about the book must be laid at the door of the publisher and not the author: the number of typographical errors is strikingly large; so large that I found it distracting, and on occasion it was not clear to me what the proper rendition ought to be. Reading is not made any easier by the fact that the typesetter has left unusually small spaces between words in many places. Most serious, however, is the garbled syntax: in a number of places, I simply could not work out what the author meant, in other places I was stopped short and had to re-read a number of times. Just a few examples at random: "She advertised the attempt of Fred Schaff, a columnist for Astronomy magazine, that cities dim their lights for hours or longer on designated days...."; "the first account of Tau Kappa Epsilon fraternity house at Alma College's being haunted appeared at Halloween time in the campus student newspaper." "The Chicago Sun-Times Action Time answered a reader's query with an epitomized history that included Candelmas candles symbolize Simeon's words to Mary that Jesus would be...." "As

the London Observer was abandoning its attempt to solve the mystery, David James was organizing the Loch Ness Phenomena Investigation Bureau, having been inspired by Constance Whyte's 1957 book, More Than a Legend, in collaboration with Richard Fitter, Sir Peter Scott and Mrs. Whyte." "Its recitation of grievances that began after an hour in the water, spent mostly in adjusting ballast and some tentative starts of the main motor, Taylor discovered that the hatch of his 20-foot do-it-yourself sub had developed a leak." "Three months later than originally planned, Agence France-Presse reported Sept. 6, 1973 that the Japanese reached London led by Shinsaku Yoshida to begin 'the biggest Loch Ness monster hunt of all times'".

Thus the meaning of what is on the page is simply not clear, far too often. In other places the chronology of the newspaper items is jumbled for no discernible or explained reason: for example, on page 260, the sequence in which stories are reported goes 10 June 1976, 11 November 1975, 20 November 1975, 24 May 1976.

I hold the publishers and not the author responsible for errors of this sort, for general and for specific reasons. First, I think any author has the right to expect that a publisher provide decent typesetting, copy-editing, and proof-reading. Second, MacDougall's eyesight has been failing; and an author of his stature and reputation surely warranted even more than normal assistance from the publisher in these technical matters. Moreover, the quality of production of the book is low: the paper creases with unusual ease; the spine of my copy showed creases and cracks within a couple of weeks; the cover warped almost immediately.

That the book remains important and useful despite its marked deficiencies is of course a tribute to the author, for his conception of the work and for making available to us the results of his remarkable collection of clippings. The contents of the book are perhaps best described by listing the headings of the chapters: Horoscopes; Astrologers; Prophecy; Doomsday; Fortune Telling; Spiritualism; Ghosts; Poltergeists and Exorcism; Luck, Good and Bad; Curses and Cures; Animals and Plants; Monsters; Sea Serpents; Loch Ness; Healing, Medical and Psychic; Witchcraft; Fundamentalism; Cults; Gurus; More Cults; Cultists; In God's Name; Artifacts and Visions; Relics; Clairvoyance; ESP; UFOs. This listing illustrates not only the range of the material but also that there is considerable overlap among the chapters. I would recommend that readers use the index of the book rather than the Table of Contents to locate items of the greatest interest to them; I found the index standing up to various tests I devised for it, which was a pleasant surprise -- at first, I had thought than an index of only four pages could not serve the purpose of such a compendium.

I do recommend the book to all readers of ZS: it should be read in short stretches, skimmed, treated with caution, but also recognized as an indispensable resource.

Champ -- Beyond the Legend. By Joseph W. Zarzynski. Bannister Publications, 1984. 224 pp. \$8.95 (paperback), \$16.95 (hardbound).

Reviewed by Henry H. Bauer

In recent years, newspapers and magazines have brought national, even international attention to the possible presence in Lake Champlain of an unidentified species of large animal. The efforts of Joseph Zarzynski over the last decade have made it respectable to report sightings; he established the Lake Champlain Phenomena Investigation, which has carried on field work and publishes a newsletter; he has interested legislators in extending legal protection to the species; and now he has written a book which makes it possible for all interested people to acquire an informed opinion about the state of the evidence for the existence of "Champ". Zarzynski has put in his debt all who have interest in anomalous claims, and cryptozoologists in particular.

The book surveys what has been said and done about Champ, makes frequent reference to Loch Ness, and puts the matter into the general context of cryptozoology. The most substantial body of data is in Appendix 4: 224 reported sightings are listed, with mention of the salient details and citation of the sources. The bibliography of four pages makes no claim to be exhaustive, but the serious student will glean additional references from the body of the text and from the list of sightings. Profuse illustrations add much interest to the book.

Claims of anomalies evince interest among many people, who I shall class approximately into three groups: those who are readily inclined or predisposed to believe, those who readily dismiss anything not firmly established, and those who are initially uncommitted as well as those interested as much in the sociology of these controversies as in the truth of specific claims. Zarzynski writes unashamedly as a believer, and that is likely to limit his appreciative audience to the aficionados and the disinterested observers; skeptics will not find here a closely argued case, with careful weighing of evidence and cautious suggestion of a possibility, and they are likely to give the matter short shrift because of the unabashed partisanship of the presentation.

An impressive aspect of the reported sightings is the contemporaneous documentation of a dozen-and-a-half instances from the 19th century (one in 1819 and the others from 1873 on). That makes a stronger historical case than has so far been constructed for Loch Ness, where sightings before 1930 are either dubious or were written up only after 1930. On the other hand, the case for Nessie has been immeasurably strengthened by film, still photographs, and sonar results; such support is not available for Champ.

Zarzynski mentions the existence of several photographs purportedly of Champ (pp. 24, 47, 61, 129, and sightings nos. 147, 164, 167, 169, 182, 198, 216), but gives no detailed description or discussion even of the ones he has seen (page 61): he rates the Mansi photo of 1977 as "the classic Champ photograph", and deals fully only with it; Appendix 2 and 3 are analyses of the photo respectively by Dr. B. Roy Frieden (Optical Sciences Center, University of Arizona) and by Paul H. LeBlond (Department of Oceanography, University of British Columbia). According to Zarzynski (p.65) and J. Richard Greenwell (p.132), the Mansi

photograph bears comparison with the Surgeon's photo at Loch Ness; but the Nessie has a much smaller ratio of head-length to neck-length, it seems to me. Moreover, the width of the apparent neck in the Mansi photograph is about 1/8 of the length of the whole object; if the latter is indeed (p.143) between 4.8 and 17.2 meters, then the diameter of the neck is between 22 and 78 inches -- and even the lower bound seems large by Nessie standards.

Comparisons with Loch Ness are made also in other places in the book; with, it seems to me, as little specific justification. The listed sightings include a goodly number of descriptions as "snake-like", which has never been said of Nessie; smooth skin is reported whereas Nessie's is rough, warty; eyes are featured several times, and fins and manes, which are almost totally lacking in reports from Loch Ness. So the comparison is hardly compelling, and I suspect it is made more for general than for particular reasons: the evidence for Nessie is so strong that it is tempting to validate other lake monsters by associating them with Loch Ness. Also, perhaps, because data about Champ is so sparse there is the temptation to augment it by extrapolating from Nessie. How sparse the information is may be seen from Chapter 7, "What is Champ?" -- for this is actually a general discussion of what lake monsters might be rather than an assessment specifically of Champ's idiosyncratic characteristics. Attempted validation by reference to Nessie is evident also in the comparison of the Champ seminar of 1981 (page 125) with the presentation of 1975 -- about Loch Ness -- in the Houses of Parliament.

For this reviewer, the existence of Champ remains to be established; but my gratitude to Zarzynski is immediate, for enabling me to find out what has been going on at Champlain and the present status of the research. The book is easy to read, and I recommend it heartily to all but the hardened skeptics and determined debunkers.





# BOOKS BRIEFLY NOTED

- \* Listing here does not preclude later full review.
- \* Critical annotations are by Marcello Truzzi.

Ainsworth, G.C., Introduction to the History of Medical and Veterinary Mycology. New York: Cambridge University Press, 1987. 228+xi pp. \$49.50. A history of the speciality dealing with fungi caused diseases. The chapter on hallucinogenic fungi should be of special interest to some ZS readers.

Albers, Donald J., and G.L. Alexander, eds., Mathematical People: Profiles and Interviews. Boston: Birkhäuser, 1985. 372+xxvi pp. \$24.95. Primarily fascinating profiles (mostly in the form of interviews) with 25 persons prominent in mathematics, including psi-critics Martin Gardner and Persi Diaconis, revealing a great deal about the minds and ideas of mathematicians including surprising differences of opinion on many issues.

Andrews, George C., Extra-Terrestrials Among Us. St. Paul, Minn.: Llewellyn Publications, 1986. 306 pp. \$9.95 paperback. A book that will infuriate both serious ufologists and critics of the extra-terrestrial hypothesis for its uncritical discussions, but Andrews has brought together much relatively obscure Fortean which I found fun even if dubious.

Angelo, Joseph A., Jr., The Extraterrestrial Encyclopedia: Our Search for Life in Outer Space. New York: Facts On File, 1985. 254+x pp. \$24.95. A nicely illustrated survey. Mistakenly equates the "UFO hypothesis" with the extraterrestrial hypothesis (ETH), but excellent in other SETI (search for extraterrestrial intelligence) areas.

Aron, Elaine and Arthur, The Maharishi Effect: A Revolution Through Meditation. Walpole, N.H.: Stillpoint, 1986. 177+xi pp. \$9.95 paperback. A remarkable volume presenting evidence that groups of persons meditating can reduce crime rates, traffic accidents, international tensions, and even affect the stock market. Though methodologically not convincing, the results warrant future efforts to replicate given the potential importance should there be substance to the claims.

Asano, Hachiro, Hands: The Complete Book of Palmistry. New York: Japan Publications, 1985. 203 pp. \$13.95 paperback. An unusual palmistry book in that it truly seeks to present a more-or-less scientific case (including some alleged research data which is too sketchy to properly evaluate) for chiromancy. Unconvincing, but certainly a step in the right direction.

Ashby, Robert H. (revised and edited by Frank C. Tribbe), The Ashby Guidebook for Study of the Paranormal. York Beach, Maine: Samuel Weiser, 1987. 215+xviii pp. \$10.95 paperback. A revised edition of the 1972 work, published in collaboration with the Spiritual Frontiers Fellowship. A useful but extremely one-sided work prepared for the metaphysically oriented and almost completely disregards the critical scientific literature. Despite such obvious shortcomings, it is a good sourcebook for addresses and other information about many of the books annotated.

Auerbach, Loyd, ESP, Hauntings, and Poltergeists: A Parapsychologist's Handbook. New York: Warner Books, 1986. 483 pp. \$4.50 paperback. A very useful and reasonable work that should help many "bothered" by ghostly phenomena as well as any would-be ghostbusters.

Barber, Theodore X., Nicholas P. Spanos, and John F. Chaves, Hypnotism: Imagination and Human Potentialities. Elmsford Park, N.Y.: Pergamon Press, 1979. 189+ix pp. No price indicated, paperback. An important updated presentation of Barber's position that there is no such thing as a special "hypnotic state." This book is especially concerned with research into and analysis of unusual cognitive processes including trance logic, hallucination, and perceptual and physiological alterations, and also including materials on stage hypnosis and acupuncture. Despite its largely discrediting findings, there is a strong positive emphasis on broadening our human potentialities. Recommended.

Barrow, John D., and Frank J. Tipler, The Anthropic Cosmological Principle. New York: Oxford University Press, 1986. 706+xxii pp. \$29.95. An extraordinary, highly controversial but fascinating and erudite work by an astronomer and a mathematician/physicist presenting a modern teleological viewpoint of the universe based on arguments from cosmology, quantum and astrophysics, biochemistry and other science sources. Of special interest is the chapter arguing against extraterrestrial intelligent life. A very important interdisciplinary work. Highly recommended.

Bauer, Henry H., Beyond Velikovsky: The History of a Public Controversy. Champaign: University of Illinois Press, 1985. 354+xiii pp. \$21.95. An exceptional study of the theories and criticisms of Immanuel Velikovsky which recognizes the severe problems with Velikovsky but does not spare the sometimes outrageous and perhaps equally pseudoscientific critics. An important case study of the controversy and a significant contribution to the sociology of science. A thoroughly scholarly and fair-minded work that should serve as an example to others concerned with anomaly controversies. Highly recommended.

Bauer, Henry H., The Enigma of Loch Ness: Making Sense of a Mystery. Urbana: University of Illinois Press, 1986. 245+xiii pp. \$22.95. Perhaps the definitive work on the Nessie controversy, sympathetic but well balanced and bibliographically indispensable. Highly recommended.

Ben-Yehuda, Nacham, Deviance and Moral Boundaries: Witchcraft, the Occult, Science Fiction, Deviant Sciences and Scientists. Chicago: University of Chicago Press, 1985. 260+x pp. \$25.00. A very important study in the sociology of science that should interest many ZS readers. Ben-Yehuda argues for the importance of deviance in relation to establishing moral boundaries in changing communities, including science, but he also emphasizes that the aims and values of science generate speculations which may in turn move the boundaries between conventional and deviant science. Highly recommended.

Berlinksi, David, Black Mischief: The Mechanics of Modern Science. New York: William Morrow, 1986. 344 pp. \$17.95. A most interesting and skeptical journey through the laboratories and seminar rooms of science raising serious questions about the pseudoscientific character of work being conducted in linguistics, mathematics and computer science (artificial intelligence) claiming that much of this work is based on an outmoded mechanistic/Newtonian view of physics. ZS readers will especially enjoy Berlinksi's comments about astrology and other rejected scientific research programs.

Björn, Nils, Problems of Mysticism. Oslo, Norway: Scintilla Press, 1980. 368 pp. No price indicated. An interesting and well done work seeking to bridge mysticism and science, arguing for a secularization of mysticism, which the author does not see as merely relevant to religious philosophy but also to other disciplines including logic and semantics. Basically a scholarly scientific/philosophic look at the literature on mysticism, the author covers a large territory including the relationship between mysticism and the issue of life after death. Should be of special interest to those ZS readers interested in psychical research.

Blackmore, Susan, The Adventures of a Parapsychologist. Buffalo, N.Y.: Prometheus, 1986. 249 pp. \$19.95. A remarkable memoir by a (mostly) disillusioned parapsychologist, hailed by many psi critics for its candor and upsetting to some psi proponents as a turnabout. Yet, Dr. Blackmore's book is more a story of her movement from perhaps too eager and premature a commitment towards the psi hypothesis into what she clearly indicates is her current agnosticism rather than (as some seem to misperceive) a new hard-line disbelief. Those familiar with the psi scene will find much of interest in this gossip and personal look at that community. (Alas, absence of an index will frustrate finding quick references to personalities mentioned.) In addition, there are many small items worth catching (e.g., Eric Dingwall's assertion to her that W.J. Crawford told him that all of Crawford's work on physical mediumship was actually quite faked). Despite her disenchantment with psi research, I suspect her ambivalence and great curiosity will bring her back into the field -- and it will be most interesting to see what the critics who have so praised her will say should she eventually get positive (psi) results.

Blackmore, Susan J., Beyond the Body: An Investigation of Out-of-Body Experiences. North Pomfret, Vt.: William Heinemann (distributed by David and Charles, Inc.), 1984. 272+xv pp. \$18.00. This is the U.S. edition of the British book reviewed in ZS#10. An outstanding and extremely important critical study that needs to be read by anyone seriously interested in OOBs, and this edition is most welcome.

Blackstone, Harry, Jr., with Charles and Regina Reynolds, The Blackstone Book of Magic and Illusion. New York: Newmarket Press, 1984. 230+x pp. \$19.95. A lavishly illustrated and highly informative book on conjuring, especially re the history of the Blackstones. Especially valuable for ZS readers for its section "The Science of Illusion" which deals with the whys and hows of magic and the psychological principles involved. Recommended.

Bletzer, June G., The Donning International Encyclopedic Psychic Dictionary. Norfolk/Virginia Beach, Va.: Donning, 1986. 875+xiv pp. A large compendium of terminologies found in the literature of "metaphysics," the occult, and the "holistic disciplines" which is useful for those areas but is rather awful when it comes to parapsychology and the scientific areas of psychical research. Ms. Bletzer clearly comes at her topics from the "metaphysical" and "New Age" corner and seems relatively unfamiliar with scientific research programs in these areas. Thus, for example, "extrasensory perception" is equated with telepathy, ignoring clairvoyance and precognition, despite citing Rhine for the term's origin.

Boerstler, Richard W., Letting Go: A Holistic and Meditative Approach to Living and Dying. So. Yarmouth, MA: Associates in Thanatology (115 Blue Rock Rd., 02664), 1985. 49+xii pp. \$3.95 paperback. A practicing thanatologist's guide to non-denominational meditative techniques to ease the anxieties and helplessness felt by the dying and others.

Bowyer, J. Barton, Cheating: Deception in War & Magic, Games & Sports, Sex & Religion, Business & Con Games, Politics & Espionage, Arts & Science. New York: St. Martin's Press, 1982. 439+xii pp. \$14.95. Not only a survey of deception and con games but presents a general theory of considerable interest. Basically a popular book full of interesting information about cheating behavior in various fields including a lengthy discussion on cheating within the field of magic (conjuring) and a number of items relevant to anomalists and psychical researchers.

Brandon, Ruth, The Spiritualists: The Passion for the Occult in the Nineteenth and Twentieth Centuries. New York: Alfred A. Knopf, 1983. 315+xiii pp. \$16.95. Also published in a \$11.95 paperback edition in 1984 by Prometheus Books. A very well written but occasionally inaccurate hard-line skeptical history of spiritualism and psychical research. Brandon seems to have ignored the critical studies of the critics themselves.

Braude, Stephen E., The Limits of Influence: Psychokinesis and the Philosophy of Science. New York: Routledge & Kegan Paul, 1986. 311+xiv pp. \$39.95. A careful examination and impressive (though to me not convincing) defense of the mostly non-experimental evidence for physical mediumship and macro-PK and examination of the major philosophical issues involved. A very provocative work on what many (including some within parapsychology) have prematurely branded a disreputable topic.

Brinberg, David, and Joseph E. McGrath, Validity and the Research Process. Beverly Hills, Cal.: Sage Publications, 1985. 175 pp. \$19.95. An important work unpacking and analyzing the concept of theory validity, showing it to be multidimensional (validity as value, correspondence and/or robustness), and presenting a Validity Network Schema (VNS) or general system for research assessment in a variety of fields. The VNS should also have applications in meta-analysis for bodies of literature such as that on various anomalies.

Brown, Hanbury, The Wisdom of Science: Its Relevance to Culture and Religion. New York: Cambridge University Press, 1986. 194-ix pp. \$32.50 hardbound; \$13.95 paperback. A grand overview, probably of particular interest to ZS readers for its discussion of the religious dimensions of science.

Brown, Laurie M., and Lillian Hoddeson, editors, The Birth of Particle Physics. New York: Cambridge University Press, 1986. 412+xxii pp. \$18.95 paperback. A collection of essays, discussions, and commentaries from a 1980 international symposium at the Fermi National Accelerator Laboratory. Of special interest to historians of physics.

Brown, Michael F., Tsewa's Gift: Magic and Meaning in an Amazonian Society. Washington, D.C.: Smithsonian Institution Press, 1986. 220 pp. \$19.95. An anthropological ethnography detailing the uses and meaning of magic among the Aguaruna Indians of northern Peru in which the author argues that magic and technology among the Aguaruna are two aspects of the same world.

and emphasizes the instrumental rather than merely expressive functions of magic. He argues that "The procedures we call 'magic' are more than a system of signs, a form of social action, or a kind of rhetoric. Not only do they speak, they explain and explore" (p. 177).

Burke, James, The Day the Universe Changed. Boston: Little, Brown and Co. 1986. 352 pp. \$27.50. Companion volume to the television series of the same name, the volume argues that reality itself has changed as knowledge has changed as James takes a strongly relativistic stance much like the "strong program" in the sociology of science. His comments on many "occultisms" like astrology should be of special interest to ZS readers.

Burke, John G., Cosmic Debris: Meteorites in History. Berkeley: University of California Press, 1986. 445+x pp. \$45.00. A thorough and impressive work of special relevance for its chapter on the early controversy.

Cairns-Smith, A.G., Seven Clues to the Origin of Life: A Scientific Detective Story. New York: Cambridge University Press, 1987. 1131+xii pp. \$8.95 paperback. A cleverly presented argument for the "clay-life hypothesis" entertainingly set forth in the form of Sherlockian reasoning. A heavy meal delightfully served.

Cairns-Smith, A.G., and H. Hartman, editors, Clay Minerals and the Origin of Life. New York: Cambridge University Press, 1986. 193 pp. \$34.50. Papers from a 1983 Glasgow conference on the "clay-life hypothesis" that the first organisms on earth used clay minerals as biomaterials. Includes papers on clay materials in meteors and on Mars and on clay as possibly the first genetic materials.

Carpenter, Kenneth J., The History of Scurvy & Vitamin C. New York: Cambridge University Press, 1986. 288+viii pp. \$39.50. A remarkable and enlightening history which places much of the current controversy about "super-health" dosages into context. Particularly interesting in its documentation about the alternative theories about causes of scurvy and the repeated discovery and forgetting of the connection between scurvy and citrus such that understanding of the disease was actually less confused in 1800 than it was in 1900.

Caughey, John L., Imaginary Social Worlds: A Cultural Approach. Lincoln: University of Nebraska Press, 1984. 280+viii pp. \$16.95. A cultural approach to imaginary social experiences including in dreams, fantasies, memories, hallucinations, etc. Valuable for its perspective on much occult-related phenomena including mediumship, alien contacts, etc.

Cazenave, Michael, editor (translated by A. Hall and E. Callander), Science and Consciousness: Two Views of the Universe. New York: Pergamon Press, 1984. 423+xvi pp. \$70.00. An extremely broad ranging series of papers from a 1979 conference on consciousness (held in Cordoba, Spain, and sponsored by the France-Culture and Radio-France Colloquium) that included prominent international figures including quantum physicists, neurophysiologists, analytical psychologists, anthropologists, philosophers and theologians, all concerned with consciousness from a variety of standpoints. A collection of special importance for those interested in the role of consciousness in quantum mechanics since it includes papers by Brian Josephson, David Bohm, Costa de Beauregard, Fritjof Capra,

Richard Mattuck and Franco Selleri, among others.)

Charig, Alan, A New Look at the Dinosaurs. New York: Facts on File, 1985. 160 pp. \$9.95 paperback. A welcome U.S. reprinting of an excellent overview, beautifully illustrated by the Curator of Fossil Amphibians, Reptiles and Birds at the British Museum (Natural History). Should be of special interest to cryptozoologists.

Chauvin, Rémy, Parapsychology: When the Irrational Rejoins Science. Jefferson, N.C.: McFarland & Co., 1985. 164+xi pp. \$18.95. This important third book on parapsychology by the eminent French biologist not only surveys new developments in parapsychological research (including work in the Soviet Union) but is especially worth of our attention for his chapter on animals and psi and his argument that we need to bring ethological and not just psychological methodology to the study of psi.

Chicorel, Marietta, editor, Chicorel Index to Parapsychology and Occult Books. Chicorel Index Series, Vol. 24. New York: Chicorel Library Publishing Corp., 1978. 354 pp. \$95.00. A useful but eccentric bibliography of materials. For example, Colin Wilson's The Occult is listed under "Lycanthropy" while Wade and Wedick's Dictionary of Spiritualism is under "Occult" and Aleister Crowley's The Equinox of the Gods is under "Spiritualism." I am still trying to figure out why John Campbell and Trevor Hall's Strange Things is listed under "U.F.O.s." One must look through every category to find a work needed since the editor obviously is unfamiliar with the contents of most of the books listed. Unfortunately, there is no author index, which would certainly have helped to correct such errors. I hope the next editions include an author index and a thorough re-categorization according to actual content of the books.

Childress, D. Hatcher, compiler, The Anti-Gravity Book. Stelle, Ill.: Publisher's Network/Adventures Unlimited Press, 1985. 209 pp. \$9.95 paperback. A remarkable compendium of anti-gravity fact and fiction from Tesla through psychotronics and UFOs.

Clow, Barbara Hand, Eye of the Centaur: A Visionary Guide into Past Lives. St. Paul, Minn.: Llewellyn Publications, 1986. 243 pp. \$9.95 paperback. An unusual book, difficult to classify, since it uses "the metaphor of reincarnation" to reveal "the way in which different levels of consciousness create mythic scenarios to join current personal life experiences with universal experiences. Controversial (if not dubious) historical sources and more phenomenological and quasi-metaphysical than scientific, but fun if not enlightening if you are into things like Egyptian deities, the druids, and Enochian and Thracian divination.

Coe, Michael, Dean Snow, and Elizabeth Benson, Atlas of Ancient America. New York: Facts on File, 1986. 240 pp. \$35.00. An excellent and lavishly illustrated guide which should be very useful for background to all ZS readers interested in archaeological anomalies like the Nazca lines, "lost" cities, etc., ranging from the Arctic to Tierra del Fuego.

Cohen, Daniel, The Encyclopedia of the Strange. New York: Dodd, Mead and Co., 1985. 291+xii pp. \$16.95. Essentially a hodge-podge of Fortean to complement Cohens other recent "encyclopedias" on ghosts and monsters. Generally accurate (though a few embarrassing errors like spelling William R. Corliss's name "Corless"), skeptical but fair-minded, and always

highly readable, this compendium is an excellent addition to Cohen's dozens of other works on related topics. An excellent starting point for anyone interested in the topics covered.

Cohen, I. Bernard, Revolution in Science. Cambridge, Mass.: Belknap Press of Harvard University Press, 1985. 711+xx pp. \$25.00. An important work in the history of science of special interest to ZS readers for its early sections dealing with failed revolutions such as N-rays and polywater.

Coleman, Loren, Curious Encounters: Phantom Trains, Spooky Spots, and Other Mysterious Wonders. Boston: Faber and Faber, 1985. 166 pp. \$11.95 paperback. An excellent collection of modern Fortean by a leading field investigator.

Collins, Andrew, The Brentford Griffin: The Truth Behind the Tales. Wickford, Essex: Earthquest Books (19 St. Davids Way; SS11 8EX), 1985. 42 pp. 1.25 pounds, paperback. Essentially a sympathetic debunking of a remarkable British episode that involved over eager earth mystery enthusiasts, the media, and perhaps even "the cosmic joker." A marvelous case study.

Collins, H.C., Changing Order: Replication and Induction in Scientific Practice. Beverly Hills, Cal.: Sage Publications, 1985. 187+viii pp. \$12.50 paperback. An important work from the perspective of the "strong program" in the sociology of science. Of special value to anomalists concerned with the social negotiation of what constitutes replication in science. Recommended.

Colombo, John Robert, Colombo's Book of Marvels. Toronto, Ontario: NCS Press, 1979. 214 pp. \$4.99 (Canadian), paperback. An alphabetically presented series of exotic events, people and places, including many true anomalies, present in Canadian history and lore. Ranges from Canadian Fairies in the Northwest to the cannibal Wendigo. Especially good on lake monsters including such odd ones as the hapyxelor in Muskrat Lake.

Cooter, Roger, The Cultural Meaning of Popular Science: Phrenology and the Organization of Consent in Nineteenth Century Britain. New York: Cambridge University Press, 1985. 418+xiv pp. \$37.50. An important work in the history/sociology of science that takes a contextualist view of science and argues that science played a crucial role—especially among the working class radicals—in the popular thought and culture of Pre-Darwinian Britain.

Corliss, William R., The Sun and Solar System Debris: A Catalog of Astronomical Anomalies. Glen Arm, Md.: The Sourcebook Project (P.O. Box 107; 21057), 1986. 282+vi pp. \$15.95. Another excellent volume in the outstanding series by the greatest scientific anomalist of our day. Essential for any serious anomaly student, this series has my highest recommendation.

Corliss, William, compiler, Earthquakes, Tides, Unidentified Sounds, and Related Phenomena: A Catalog of Geophysical Anomalies. Glen Arms, Md.: Sourcebook Project, 1983. 214+vi pp. \$12.95. Another indispensable volume in Corliss's masterful series on anomalies. Highly recommended.

Cranston, Sylvia, and Carey Williams, Reincarnation: A New Horizon in Science, Religion and Society. New York: Julian Press, 1984. 385+xv pp. \$16.95. A very good popular survey with a great deal of fascinating material. Not a scientific effort (e.g., it ignores the problem for reincarnation theory posed by the demographic fact that about 50% all the persons born since 6000 B.C. have lived in the last 75 years, thus making it impossible for all of them to have had multiple past lives in the last 8000 years), but full of historical and philosophical details of likely interest to many ZS readers.

Crow, Michael J., The Extraterrestrial Life Debate 1750-1900: The Idea of a Plurality of Worlds from Kant to Lowell. New York: Cambridge University Press, 1986. 680+xix pp. \$59.50. An outstanding scholarly work showing the widespread roots of the debate about extraterrestrial life and its impact upon many areas of intellectual history. An important work in the history of science which largely takes off from where Steven J. Dick's 1982 work, The Plurality of Worlds: The Extraterrestrial Life Debate from Democritus to Kant ends.

Culver, R.B., and P.A. Ianna, The Gemini Syndrome: A Scientific Evaluation of Astrology. Buffalo, N.Y.: Prometheus Books, 1984. 222 xii pp. \$18.95 hardbound, \$11.95 paperback. A revised edition of their 1979 book, mainly including an updating final chapter but, alas, the index was not updated to include it. Generally an excellent work critical of astrology but somewhat inadequate in its consideration of the Gauquelin research program.

Cunningham, Scott, The Magic of Incense, Oils and Brews: A Guide to Their Preparation and Use. St. Paul, Minn.: Llewellyn, 1986. 165+xv pp. \$6.95 paperback. A general compendium of recipes for magical events, purportedly authentic or "tested" by "research" of the author, who has previously written two other books on herbal magick. At least nothing seems present in these recipes likely to harm the users, but some readers will be disappointed to find no cursing or "evil" recipes herein.

Curran, Douglas, In Advance of the Landing: Folk Concepts of Outer Space. New York: Abbeville Press, 1986. 132 pp. \$16.95 paperback. A marvelous collection of photographs of and text about flying saucer folks from builders of exotic craft to contactees, including a Foreword by Tom Wolfe.

Davis, Wade, The Serpent & the Rainbow. New York: Simon and Schuster, 1986. 299 pp. \$17.95. A Harvard ethnobotanist's anthropological investigation into Haiti's Vodoun culture which resulted in his being able to obtain the formula for the drug used to produce zombie workers. Fascinating stuff which reinforces my conviction that we should look for fire under all the legendary smoke rather than dismiss it all as mere smoke.

Dick, Steven J., Plurality of Worlds: The Origins of the Extraterrestrial Life Debate from Democritus to Kant. New York: Cambridge University Press, 1984. 246+x pp. \$13.95 paperback. A fascinating study in the history of science (involving such notables as Aristotle, Aquinas, Ockham, Galileo, Kepler, Huygens and Kant) presenting the path of convergence in early scientific thought towards the opinion that intelligent life must fill the universe. Highly recommended.

Domhoff, G. William, The Mystique of Dreams. Berkeley: University of California press, 1985. 146+x pp. \$14.95. A critical examination of the dream ideas about dream sharing and control that were attributed to the Malaysian tribe of the Senoi by Kilton Stewart and which gained wide popularity in the 1960's. In addition to discrediting these claims, Domhoff places the events in sociological perspective and argues that the acceptance of these notions tells us more about American culture than it does about the Senoi.

Dreyfus, Hubert L., and Stuart E. Dreyfus, Mind Over Machine. New York: The Free Press, 1986. 231+xviii pp. \$16.95. An important book by two computer scientists debunking the hoopla equating human and artificial (machine) intelligence and the many misuses of computer models in education, management, medicine, defense and elsewhere. Presenting the major arguments against misguided attempts to replace the human mind with machine-like logic, the book is an excellent antidote to the overzealous "computer pundits and their entrepreneurial allies."

Duerr, Hans Peter (translated by Felicitas Goodman), Dreamtime: Concerning the Boundary between Wilderness and Civilization. New York: Basil Blackwell, 1985. 462+xii pp. \$24.95. A remarkable and enormously erudite anthropological essay (the notes constitute about 2/3 of the total text) which argues that all cultures set up boundaries that allow them to define daily realities but that they also create certain rituals for transcending or crossing over those boundaries into the "wilderness" of untamed nature. Duerr argues that full understanding can come only with the occasional crossing of such boundaries and that modern scientific pragmatism which seeks to eradicate the "wilderness" is limiting our vision.

Dychtwald, Ken, Bodymind. Los Angeles: Jeremy P. Tarcher, 1986. 302+17 pp. \$8.95 paperback. A revision of the very popular 1977 holistic health work by a humanistic psychologist who builds upon ideas from Ida Rolf, Wilhelm Reich, Alexander Lowen, and Eastern philosophy.

Dyson, Freeman, Origins of Life. New York: Cambridge University Press, 1985. 81+ix pp. \$7.95. The Turner Lectures given at Trinity College, over-viewing the theories and experiments, Dyson's own mathematical model, and an excursion into the philosophical issues,

Eberhart, George, UFOs and the Extraterrestrial Contact Movements: A Bibliography, 2 volumes. Metuchen, N.J.: Scarecrow Press, 1986. 1,298+xxv pp. \$97.50. An extraordinary compilation covering not only general UFO analyses, periodicals and case studies in 35 countries but many related phenomena such as sonic booms, ancient contact, hollow earth, Marian apparitions, and extraterrestrial intelligence. Over 15,000 entries, scrupulously cataloged and cross-indexed. Absolutely essential for anyone seriously concerned with researching ufology and useful for many other anomaly areas as well. If you can not afford it (and it is well worth the price), get your library to obtain it. Like Eberhart's other fine anomaly bibliographies, this is a pleasure to use and to browse in. All anomalists owe Eberhart a great debt for his Herculean labors that go a long way in advancing anomalistics as a scholarly enterprise. Highly Recommended.

Edge, Hoyt L., Robert L. Morris, Joseph H. Rush and John Palmer, Foundations of Parapsychology: Exploring the Boundaries of Human Capability. Boston: Routledge & Kegan Paul, 1986. 432+xvi pp. \$49.95 clothbound; \$22.50 paperback. Despite areas of omission and the problems of multiple authorship, this is probably the best current general but advanced text/review on contemporary parapsychological research. Certainly, this is the book I would recommend for reading by psychologists inclined to dismiss psi research. Recommended.

Eisenberg, David, with Thomas L. Wright, Encounters With Qi: Exploring Chinese Medicine. New York: W.W. Norton, 1985. 254 pp. \$16.95. A physician's survey of his investigations into contemporary interest and research into Qi (the vital energy source; pronounced "chee") and Qi Gong (the "aerobic" exercise program) as well as other areas of traditional medicine practiced in the People's Republic of China. The author seems somewhat naive in regard to fraudulent methods probably involved in some of the paranormal phenomena being claimed, but the book is generally conservative and recognizes the need for further and extended research into such matters.

Fairley, John, and Simon Welfare (with foreword and epilogue by Arthur C. Clarke), Arthur C. Clarke's World of Strange Powers. New York: G.P. Putnam's Sons, 1985. 248 pp. \$19.95. The volume expanding on and accompanying the TV series of the same name (Clarke's second such project, the earlier one being his Mysterious World). Well illustrated and generally excellent and a good balance between being skeptical and open minded, sometimes even taking issue with critics (e.g., Clarke's disagreement with Randi over the effectiveness of dowsing).

Fine, Arthur, The Shaky Game: Einstein, Realism, and the Quantum Theory. New York: Cambridge University Press, 1986. \$25.00. In addition to being a thorough study of Einstein's views on these issues, Fine argues for a "natural ontological attitude" that insists that any philosophical description of science must include a variety of scientific practices and not attempt to encompass them all in a single philosophical "ism." He argues that in the end all of science is a "shaky game" without rigid structure or universal rules.

Flew, Antony, editor, Readings in the Philosophical Problems of Parapsychology. Buffalo, N.Y.: Prometheus, 1987. 376 pp. \$24.95 hardbound; \$16.95 paperback. An interesting but strongly biased collection both in terms of its omissions (e.g., the several attacks on critics' misuses of the Humean argument against miracles and the unfalsifiable position of those like Hansel, and George Price's attack on psi is presented but no mention is made of his later retraction) and the rather dated and now largely questionable inclusions (e.g., Flew seems to accept Broad's ideas about limited principles in science which modern physics has caused many to now reconsider). Still, a useful collection worth owning as a supplement to the several other similar (and I think superior) anthologies on these issues.

Franklin, Allan, The Neglect of Experiment. New York: Cambridge University Press, 1986. 290+xiii pp. \$42.50. An important work focusing on the epistemology of the experiment, arguing for the importance of experiments in science by considering actual experiments, and including a chapter on

fraud in science which should be of special interest to many ZS readers.

Frazier, Kendrick, editor, Science Confronts the Paranormal. Buffalo, N.Y.: Prometheus Press, 1986. 367+xiv pp. \$15.95 paperback. The second collection of articles from The Skeptical Inquirer. In general, I found this collection weaker than the earlier collection. Many will find the collection flawed (as is the original journal) by the absence of critical (or balancing) perspectives on many things said in these essays. That is, the "skepticism" displayed (except for a minority of essays such as that by Toulmin) is quite one-sided. Nonetheless, this collection of mostly advocacy (pro-orthodoxy) essays is an important compendium which proponents of paranormal claims would be well advised to read.

Friedlander, Joel, Body Types, New York: Globe Books, 1986. 168 pp. \$19.95. A metaphysical work about constitutional personality types based on the teachings of Gurdjieff, Ouspensky, and others of the Fourth Way school. For those interested in a scientific approach to such matters, they might better stick with Sheldon and similar approaches; on the other hand, if one likes this set of occult concepts, I presume one could take this formulation as the basis for empirical hypotheses still to be tested rather than as the glib proclamations as presented here.

Gallup, George, Jr., Religion in America, 50 Years: 1935-1985. Princeton, N.J.: The Gallup Report, No. 236, May 1985. 57 pp. No price indicated, paperback. A report on survey information about U.S. attitudes and their shifts over the past 50 years towards a wide variety of religious ideas and practices.

Gardner, Martin, editor, The Wreck of the Titanic Foretold?. Buffalo, N.Y.: Prometheus Press, 1986. 157 pp. \$18.95. A welcome reprinting of Morgan Robertson's 1898 novel The Wreck of the Titan and other seemingly prophetic writings about the 1912 sinking of the Titanic, along with an introductory essay and informative and useful biographical/historical materials by the editor, Martin Gardner. Though Gardner dismisses the "predictions" as mostly mere coincidence, I suspect that many readers of these materials will probably disagree on the basis of the arguments and evidence Gardner presents here.

Geller, Uri,, and Guy Lyon Playfair, The Geller Effect. New York: Henry Holt & Co., 1987. 288 pp. \$17.95. This joint effort (Playfair wrote an opening and the final five chapters) give us Geller's reply to his critics and tells us of the last ten years during which Geller claims to have become a multimillionaire several times over using his "powers" to locate oil and minerals for international corporations. Geller and Playfair make some excellent points in response to Geller's critics, and overall the book is very well done given its purposes. Not all critics are answered (e.g., the Popular Photography episode is ignored), and the book is clearly not intended to do that, but it does give excellent reasons to also show skepticism towards his more virulent critics' claims that they have thoroughly debunked Geller, who seems to be laughing all the way to the bank. The bottom line is well put by Playfair (p. 219): "Geller has never been proved fraudulent. Nor has he ever been proved to have genuine psychic abilities beyond any reasonable doubt. It is possible that it never will be proved either way, and I suspect this is the way he likes it."

Giere, Ronald N., Understanding Scientific Reasoning. New York: Holt, Rinehart and Winston, 1979. 360+xii pp. \$19.95 paperback. This is a basic text for "applied philosophy" and philosophy of science courses and contains a chapter on "Fallacies of Theory Testing" that uses pseudoscientific examples of special interest to ZS readers. I also found it an excellent presentation on probability, the complexities of testing causal hypotheses, and decision theory. Recommended.

Goran, Morris and Marjorie, The Lure of Longevity: The Art and Science of Living Longer. Saratoga, Cal.: R&E Publishers (P.O. Box 2008; 95070), 1984. 160+viii pp. \$12.95 paperback. An excellent quick review of the scientific and nonscientific world of longevity belief and research, with much information of likely interest to ZS readers.

Goran, Morris, A Guide to the Perplexed About Pseudoscience: How to Recognize Science. Chicago: Cimarron Publishers, 1985. 191+i pp. \$10.00 spiral-bound. A useful but not always reliable set of materials (e.g., Upton Sinclair's book Mental Radio is given as Mind Reach, Richet is described as a believer in survival when and other boners are present) that many anomalists should find useful despite the author's simplifications of the complex demarcation problems.

Gregory, Anita, The Strange Case of Rudi Schneider. Metuchen, N.J.: Scarecrow Press, 1985. 444+xvii pp. \$24.50. An exceptional study of the great physical medium by the late Dr. Gregory, expanding her earlier Annals of Science monograph. A very welcome addition to the history of psychical research. Recommended.

Hankins, Thomas L., Science and the Enlightenment. New York: Cambridge University Press, 1985. 216+viii pp. \$29.95. A brief general history of 18th century science in its broader cultural context, tracing the emergence modern scientific fields in light of recent historical scholarship.

Hansen, Harold A., A Witch's Garden. York beach, Maine: Samuel Weiser, 1983. 128 pp. \$5.95 paperback. A translation of the Danish book on the botany of the poisonous and hallucinogenic plants found in witchcraft lore, including mandrake, henbane, deadly nightshade, thornapple, hemlock, monkshood, flying ointments, and even an appendix on the ingredients in the brew depicted in Macbeth. An excellent and authoritative work.

Harman, Gilbert, Change In View: Principles of Reasoning. Cambridge, Mass.: Bradford Books/MIT Press, 1986. 147+ix pp. \$19.95. A philosophical study of reasoning which identifies principles for revising one's beliefs other than logical principles, suggesting a radical new view of reasoned revision as a nonlinear, nonmonotonic matter of making piecemeal adjustments in response to new knowledge and intuitions. In other words, human beings do not reason in a formal logical sense at all but do so according to rather different principles which the author defends. A work of great potential importance for studies of artificial intelligence and, I should think, for cognitive psychologists concerned with human's psycho-logic.

Harman, Willis, and Howard Rheingold, Higher Creativity: Liberating the Unconscious for Breakthrough Insights. Los Angeles: J.P. Tarcher, 1984. 239+xxiv pp. \$14.95 hardbound, \$8.95 paperback. A rather uncritical but fascinating general guide put forward to lead to enhanced creativity and

increased human potential through the harnessing of inner resources some researchers believe to be present in the unconscious.

Harris, Melvin, Investigating the Unexplained. Buffalo, N.Y.: Prometheus Books, 1987. 222 pp. \$19.95. A nice debunking volume by an investigative broadcast journalist, generally reasonable in tone and examining about a dozen wondrous claims ranging from the Amityville horror-mongers to psychic detectives involved in the Yorkshire and Jack the Ripper cases to pseudoreincarnation claims. Overall, I thought the book excellent with my only real complaint being Harris's apparently being unaware that some of the sources he attacks (like the self-styled "parapsychologist" and ghost-monger Hans Holzer) have never been granted any authority by serious psychical researchers; so Harris occasionally sets up a bit of a red herring, but I think this was not intentional and still serves a useful function in a book intended for the general public which does not know the difference either.

Hart, Ernest, Hypnotism, Mesmerism & Witchcraft. Toronto, Ontario: Coles, 1980. 212+ix pp. \$2.95 (Canadian). A reprinting of the 1896 series of papers by Hart which first appeared in Nineteenth Century and the British Medical Journal. A highly critical-skeptical book. The appendix is of special interest since it reprints a number of letters from articles of the day, including an interesting set on Palladino.

Herbert, Nick, Quantum Reality: Beyond the New Physics. Garden City, N.Y.: Doubleday, 1985. 268+xv pp. \$16.95. Consideration of 8 major quantum theoretical models of the nature of the universe. An excellent introduction, especially to the implications of Bell's Theorem for our view of reality. Avoids direct consideration of parapsychological issues, but the implications are clear, especially with model #7 wherein consciousness creates reality. Recommended.

Hewstone, Miles, editor, Attribution Theory: Social and Functional Extensions. New York: Basil Blackwell, 1984. 256+xii pp. \$39.95 hardbound; \$14.95 paperback. An excellent collection of original essays of special interest to anomalists since causal attribution is a common issue and the psychology of attribution is complex and highly relevant in understanding potential sources of error in attribution.

Hobsbawm, Eric, and Terence Ranger, editors, The Invention of Tradition. New York: Cambridge University Press, 1984. 320+vi pp. \$29.95 hardbound; \$9.95 paperback. A remarkable historical work dealing with the artificial creation of historical pasts for political and nationalistic purposes. Though not directly concerned with occultism, these essays (especially the two dealing with Scottish and Welsh pseudo-traditions) should be of special value to ZS readers given the vast amount of pseudo-history found in the so-called occult traditions.

Holiday, Ted, The Goblin Universe. St. Paul, Minn.: Llewellyn Publications, 1986. 262 pp. \$9.95 paperback. This book, which is a posthumously published hodgepodge of all sorts of alleged anomalous things, is entertaining reading, but as Colin Wilson admits in his sympathetic and informative 45-page introduction, this book "would never convert a single skeptic; in fact, it would probably make him more certain than ever that 'the occult' is a farrago of self-deception and muddled thinking." Wilson is right.

Huyghe, Patrick, Glowing Birds: Stories from the Edge of Science. Boston: Faber and Faber, 1985. 241+ix pp. \$9.95 paperback. An excellent collection of science essays, highly readable, and most of which should be of special interest to ZS readers. Recommended.

Illingworth, Valerie, editor, The Facts On File Dictionary of Astronomy. New York: Facts On File Publications, 1986, second edition. 437 pp. \$19.95. An excellent general work with minimal concern with purported anomalies; thus there are listings for such main-line concerns as "exobiology," "SETI," and even "Vulcan," but no listing at all for "astrobiology" or "UFO." Nonetheless, an excellent supplementary volume for any anomalist dealing with astronomy.

Inglis, Brian, The Hidden Power. London: Jonathan Cape, 1986. 312 pp. 10.95 pounds (U.K.). Inglis is an important historian of spiritualism who in many ways reminds me of Sir Arthur Conan Doyle, often brilliant but sometimes highly credulous. The first section of this book deals with the evidence for psi. I find it deeply flawed as I do his other pro-psi histories. The bulk of this book, however, is an attack on the critics of psychical research (who Inglis characterizes as exemplifying scientism rather than science), and here I think most of Inglis's arguments hit their target effectively. Inglis is clearly an advocate for his point of view, and he needs to be read critically, but he and this book play an important role in the dialectical process that constitutes science. I would strongly recommend this book to my fellow critics as a healthy antidote to propaganda from advocates on the orthodox side.

Irwin, H.J., Flight of Mind: A Psychological Study of the Out-of-Body Experience. Metuchen, N.J.: Scarecrow Press, 1985. 374+viii pp. \$27.50. A thoroughgoing survey of the phenomenology, experiments and theories of OOBs. The author, a psychologist in Australia, offers his own sophisticated imaginal theory. Must reading for anyone concerned with OOBs.

Jacobson, Steven, Mind Control in the United States. Santa Rosa, Cal.: Critique Publishing, 1985. 72 pp. \$4.95 paperback. An introduction to the history and practice of alleged subliminal communication in a wide variety of media. Essentially a "conspiracy" orientation based on well documented but too often highly dubious sources (e.g., The National Enquirer). Nonetheless, some interesting information tracked down and worth the read.

Joravsky, David, The Lysenko Affair. Chicago: University of Chicago Press, 1986. 459+xi pp. \$15.95, paperback. A welcome reprint of the 1970 edition, this is the most authoritative and detailed treatment of this bizarre episode in the history of science.

Kassin, Saul M., and Lawrence S. Wrightsman, editors, The Psychology of Evidence and Trial Procedure. Beverly Hills, Cal.: Sage Publications, 1985. 384 pp. \$29.95. An excellent collection on the social psychology of such matters as eyewitness and character testimony, polygraph use, and many other matters of direct potential relevance for those interested in assessing anomaly claims.

Kevles, Daniel J., In the Name of Eugenics: Genetics and the Uses of Human Heredity. Berkeley: University of California Press, 1986. 426+x pp. \$9.95



paperback. An exceptional scholarly history, thoughtful and revealing, of the intertwining between the science of genetics and the political programs (often characterized as pseudosciences) of eugenics.

Khalsa, Parmatma Singh, editor, The New Consciousness Sourcebook: Spiritual Community Guide #6. 208 pp. \$8.95 paperback. A new and updated edition of what many now view as the standard directory to the metaphysical community, this issue focusing on holistic health.

Kilbourne, Brock K. and Maria T., editors, The Dark Side of Science. Proceedings of the 63rd Annual Meeting of the Pacific Division, American Association for the Advancement of Science, Vol. 1, Part 2, August 30, 1983. San Francisco: Pacific Division AAAS, 1983. 226 pp. \$8.95 paperback. An exceptional collection of 13 papers originally part of a 1982 symposium entitled "Science, Deviance, and Society." Outstanding papers dealing with fraud and other deviance in science. Highly recommended.

Kles, Cosette N., The Occult in the Western World: An Annotated Bibliography. Hamden, Conn.: Shoe String Press, 1986. 233+xii pp. \$29.50. A very useful though highly incomplete listing. Kles sometimes places works in odd categories (e.g., Slater's The Wayward Gate is among the "skeptics" books) and important distinctions are often ignored (e.g., pagan witchcraft is lumped in with Satanism). Despite its problems, an excellent work well worth obtaining.

Kohn, Alexander, False Prophets: Fraud and Error in Science and Medicine. New York: Basil Blackwell, 1987. 226+x pp. \$24.95. An excellent survey of the "dark side of science" including plagiarism, data falsification, misrepresentation of results, and misappropriation or misuse of public funds. Mainly about conventional rather than unorthodox science, Kohn's book should be of special interest to ZS readers concerned about allegations of similar misconduct among anomaly claimants.

Krippner, Stanley, editor, Advances in Parapsychological Research, Vol. 4. Jefferson, N.C.: McFarland, 1985. 254 pp. \$29.95. A most welcome addition to what has, in my opinion, been the most important series of books within psi research. Review articles on psychokinesis, mental healing, mental imagery and psi research, recent ganzfeld-ESP research, the implications of parapsychology for psychology, and a bibliographic guide to parapsychology books during 1979-82. But the highlight for me was Charles Akers' chapter on methodological criticisms of parapsychology which is probably the best critical review now available and is a wonderful demonstration of the openness to responsible criticism present within parapsychology, for this example of internal criticism is superior to nearly all the efforts by psi research's external critics.

Kunze, Michael, translated by William E. Yuill, Highroad to the Stake: A Tale of Witchcraft. Chicago: University of Chicago Press, 1987. 424+xv pp. \$24.95. A novelized but nonfictional and historical study of a vagrant German family of travellers around 1600 who were arrested and condemned for witchcraft. Entertaining and informative study of the common people too often forgotten by most historians, in this case caught up in one of histories most grotesque aberrations, the inquisition.

Kurtz, Paul, The Transcendental Temptation: A Critique of Religion and the Paranormal. Buffalo, N.Y.: Prometheus, 1986. 500+xiv pp. \$18.95. In many ways an interesting and informative book, but Kurtz's consistently fails to see that psychical research has been an attempt to naturalise the supernatural. Thus he mistakenly equates the supernatural with the paranormal (a term that was created to bring phenomena previously thought supernatural into the realm of the natural so that it could be evaluated by science). This results in the false presentation of paranormal claims as alleged "miracles" instead of as merely extraordinary events (which even Hume recognized were exempt from his arguments against miracles, a lesson most critics who invoke Hume fail to observe). Although I am inclined to agree with Kurtz's arguments against the supernatural, his lack of rigor in dealing with the paranormal (where I have some expertise) makes me cautious about accepting his seemingly fair representations of the arguments by theologians (where I have no expertise).

LaBerge, Stephen, Lucid Dreaming: The Power of being Awake and Aware in Your Dreams. Los Angeles: J.P. Tarcher, 1985. 277+x pp. \$15.95. A report on work done at the Stanford University Sleep Research Center on creating and guiding one's own dreams. A fascinating emerging area with interesting implications for our views on transcendence.

Latour, Bruno, Science In Action: How to Follow Scientists and Engineers Through Society. Cambridge, Mass.: Harvard University Press, 1987. 274 pp. \$25.00. An analysis of science in practice here produces a view of scientific networks of activity that through a "trial of strength" determines what is "objective" or "subjective." Going further, Latour argues that "Irrationality is always an accusation made by someone building a network over someone else who stands in the way; thus, there is no Great Divide between minds, but only shorter and longer networks." An important work supporting the "strong programme" in contemporary sociology of science.

Lehmann, Arthur C., and James E. Myers, editor, Magic, Witchcraft, and Religion: An Anthropological Study of the Supernatural. Palo Alto, Cal.: Mayfield, 1985. 416+xiv pp. \$19.95 paperback. An interesting collection reprinting many of the more recent papers emphasizing the contemporary scene. Recommended.

Leith, Thomas Henry, compiler, The Contrasts and Similarities among Science, Pseudoscience, the Occult, and Religion: A Bibliography, 4th Edition. Downsview, Ontario: York University (Dept. of Natural Sciences; Atkinson College), 1986. 207 pp. No price indicated, paperbound. A very welcome bibliography but a bit difficult to systematically use because of the somewhat odd arrangement of sections and the listing of articles alphabetically by title rather than author. If a new edition is forthcoming, I hope Leith will consider a new and more standard arrangement.

Leonard, George B., The Transformation: A Guide to the Inevitable Changes in Humankind. New York: J.P. Tarcher, 1987. 258+xvii pp. \$8.95 paperback. The 15th anniversary edition of a "new age classic." I wish I shared his optimism for our future.

Lester, Paul, The Great Sea Serpent Controversy: A Cultural Study. Birmingham, England: Protean Pubs (Flat 4; 34 Summerfield Crescent; Edgbaston; Bir-



- mingham B16 OER), 1984. 24 pp. 60 pence (plus postage and packing), booklet. A most interesting and provocative essay in cultural history on the meaning of the sea serpent (including the lake creatures "Nessie" and "Caddy") mythos.
- Lifton, Robert Jay, The Nazi Doctors: Medical Killing and the Psychology of Genocide. New York: Basic Books, 1986. 561+xiii pp. \$19.95. An important study of the extraordinary psychological process he calls "doubling," the forming of a second, relatively autonomous self, by the medical doctors who engaged in mass murder to "heal" what they perceived to be the racially "diseased" body of the German nation. A major study continuing Lifton's well known work on humans in extreme conditions
- Litvinoff, Sara, General Editor, The Illustrated Guide to the Supernatural. Boston, Mass.: G.K. Hall, 1986. 156 pp. \$25.00. An attractive "coffee-table" compilation but highly credulous and uncritical, and with a "foreword" by Richard Cavendish.
- Loye, David, The Sphinx and the Rainbow: Brain, Mind and Future Vision. New York: Bantam Books, 1984 edition. 319+xvi pp. \$3.95 paperback. An attempt by a prominent futurist/forecaster to integrate work in neurophysiology, physics and parapsychology towards a "new psychophysics" that will account for and allow the practical use of precognition. Of special interest is the authors appendix "On the Evaluation of the Paranormal" in which he raises some original arguments against the critics of psi.
- Lund, David H., Death and Consciousness. Jefferson, N.C.: McFarland, 1985. 194+x pp. \$18.95. A philosopher's general examination of and optimistic conclusions about the evidence and arguments for survival found in psychological research. Though fair-minded and a good introductory work, little attention is given to modern critics or relevant contemporary philosophical perspectives on the nature of mind (e.g., by the British language philosophers).
- Malony, H. Newton, and A. Adams Lovekin, Glossolalia: Behavioral Science Perspectives on Speaking in Tongues. New York: Oxford University Press, 1985. 292 pp. \$29.95. An excellent, thorough, and sympathetic review of a fascinating topic. Discredits many of the myths (believed in by both proponents and critics) surrounding glossolalia. The best book I have seen on this subject. Recommended.
- McClenon, James, Deviant Science: The Case of Parapsychology. Philadelphia: University of Pennsylvania Press, 1984. 282+xiii pp. \$14.95 paperback. An important sociological study of the legitimacy problems encountered by the protoscience of parapsychology, particularly valuable for its survey of elite science gate-keepers in the AAAS. Recommended.
- McCoy, Duke, How to Organize and Manage Your Own Religious Cult: A Psycho-Political Primer. Mason, Mich.: Loompanics Unlimited, 1980. 90 pp. \$4.95 paperback. Pretty much what its title says: a Machiavellian manual on how to be charismatic, find followers, etc. A totally amoral presentation and generally well researched.
- McNeil, W.K., compiler-editor, Ghost Stories from the American South. Little Rock, Ark.: August House, 1985. 170 pp. \$19.95 hardback, \$7.95 paperback.
- An excellent collection of folktales, collected by a leading folklorist, ranging from Tidewater Virginia to Texas.
- McRae, Ronald M., Mind Wars: The True Story of Secret Government Research into the Military Potential of Psychic Weapons. New York: St. Martin's Press, 1984. 155+xxviii pp. \$12.95. A sensationalistic and occasionally erroneous (e.g., the "Madame Zodiac" story has since been disclosed to be pure fabrication) but mostly reliable presentation of evidence (some of which may be disinformational) dug up by an investigative reporter into U.S. government psi efforts.
- Mead, Chris, Bird Migration. New York: Facts on File, 1983. 224 pp., \$19.95. A lavishly illustrated complete survey of information on bird migration including excellent review of the experimental work that has been done on its mysteries. Includes work on the "sixth sense" of magnetic character (in both birds and humans) which should be of special interest to ZS readers. Recommended.
- Melton, J. Gordon, The Encyclopedia of American Religions, First Edition Supplement. Detroit, Mich.: Gale Research, 1985. 177 pp. \$75.00 paperback. An expensive update with cumulative indexes to both volumes of the first edition of this extraordinary scholarly effort which is indispensable for any research library dealing with American religions. Melton's thoroughness and scope in monitoring variations in religion is outstanding, and many of the smaller esoteric and occult-related groups listed make especially fascinating reading (e.g., the SM Church, the Venusian Church, etc.) and have relevance for anomalists.
- Melton, J. Gordon, Biographical Dictionary of American Cult and Sect Leaders. New York: Garland Publishing, 1986. 354+xiii pp. \$39.95. A compilation of carefully checked biographical information (including bibliography) on 267 religious founders and leaders mostly unavailable elsewhere. An excellent companion to Melton's Encyclopedia of Cults in America. Recommended.
- Melton, J. Gordon, Encyclopedic Handbook of Cults in America. New York: Garland Publishing, 1986. 272+x pp. \$24.95. An outstanding and dispassionate historical and current survey of the leading cult and anti-cult organizations, including pagan, spiritualist, New Age, and most other such old and new groups. A wealth of objective information (the very few errors I spotted were due to mistakes in the sources Melton cites), and especially valuable for Melton's sections on "What is a cult?" and the "Counter-cult Groups."
- Michel, John, Eccentric Lives & Peculiar Notions. New York: Harcourt Brace and Jovanovich, 1984. 240 pp. \$15.95. An entertaining gallery of some outrageous historical and contemporary characters, including some flat-earthers, druids, Baconians, ufologists and other fun people.
- Minsky, Marvin, The Society of Mind. New York: Simon and Schuster, 1987. 339 pp. \$19.95. A comprehensive theory of mind by a leading authority on artificial intelligence, presenting a holistic view of the mind's learning stages in an unusually clear (and surprisingly nontechnical) presentation

- Monroe, Robert A., Far Journeys. Garden City, N.Y.: Doubleday, 1985. 292+xi pp. \$15.95. The author claims to have developed a revolutionary sound wave process known as Hemi-Sync which produces a state of consciousness that facilitates out-of-body experiences which include travel not only in space but in time. Interesting but scientifically unconvincing and poor methodologically.
- Montgomery, Ruth, with Joanne Garland, Ruth Montgomery: Herald of the New Age. Garden City, N.Y.: Doubleday, 1986. 277+ix pp. \$16.95. Another potboiler by what the publisher calls "the world's psychic authority" and the "First Lady of the Psychic World." Typical of the genre (wherein an author begins by writing a first book in which he/she claims to have started out a "skeptic" but discovered the powers of the psychic who is the book's subject --Jeane Dixon in Montgomery's case-- then writes about new psychics met, then discovers how to develop psychic powers, and finally writes books in which he/she is the new guru), Montgomery's book goes a step further in appearing "objective" since this book is written in the third person (presumably by Ms. Garland) even though Montgomery is listed as its author. Some readers will be pleased to learn that Montgomery's guides seem to stick with her earlier prediction that Ronald Reagan will not make it through his second term and also predict that there will be no World War III between now and the end of the 1990s. Wheew, I had started to worry...
- Morgan, Chris, and David Langford, Facts and Fallacies: A Book of Definitive Mistakes and Misguided Predictions. New York: St. Martin's Press, 1981. 176 pp. \$9.95. A most interesting collection of remarkably wrong statements by all sorts of people, mostly famous, putting their feet in their mouths by mistaking the future. Particularly valuable for the dogmatic statements about various impossibilities by famous scientists. The weakness of the book is that it gives the authors' names but not the sources for the quotations.
- Nalimov, V.V. (edited by R.G. Coldny and translated by A.V. Yarkho), Realms of the Unconscious: The Enchanted Frontier. Philadelphia, Penn.: ISI press, 1982. 320+xvii pp. \$29.50. A remarkable book by a Soviet cybernetic theorist at Moscow State University, in which he surveys the features of consciousness which cannot be assimilated or simulated by computer. Surprising citations including some to Blavatsky, Carlos Castaneda, and Charles Tart. An attempt at synthesizing elements of semantics, science, and mysticism into a new view of human perception, extending his earlier two volumes in this trilogy, which developed a cybernetic model of the real, going into a theory of the unreal. Should be of special interest to many ZS readers since it even touches on reincarnation beliefs among its many topics relevant to consciousness and claims of the paranormal.
- Nash, Carroll B., Parapsychology: The Science of Psiology. Springfield, Ill.: Charles C. Thomas, 1986. 336+vii pp. \$28.79 paperback. A very substantially rewritten and updated version of the author's earlier Science of Psi, intended as a textbook for parapsychology. The main problem with the work is its clear commitment to the existence of psi, thus predicating "psiology" which goes well beyond the position of parapsychology where psi remains an hypothesis and the major investigation is into the psi experience (whether or not psi is valid).
- Oldroyd, David, The Arch of Knowledge: An Introductory Study of the History of the Philosophy and Methodology of Science. New York: Methuen, 1986. 413 pp. \$12.95 paperback. A very welcome and excellent survey which should be of special interest to ZS readers. Balanced and lucid coverage of modern philosophers of science like Wittgenstein, Popper, Lakatos and Feyerabend is especially good as an introduction to their approaches. Recommended.
- Otten, Charlotte F., editor, A Lycanthropy Reader: Werewolves in Western Culture. Syracuse, N.Y.: Syracuse University Press, 1986. 337+xvi pp. \$14.95 paperback. An excellent survey featuring classic (ancient but mainly medieval and Renaissance) and modern studies including medical, criminal and legendary reports. Recommended.
- Oyama, Susan, The Ontogeny of Information: Developmental Systems and Evolution. New York: Cambridge University Press, 1986. 206+ix pp. \$34.50 clothbound; \$22.95 paperback. A constructivist view of the nature-nurture complex arguing that ontogenetic "information" does not exist in the genes or in the environment but is constructed in a given developmental context; in contrast to the usual view in terms of genetic necessity and cultural overlay. The chapter on "the ghosts in the ghost-in-the-machine machine" should be of special interest to those ZS readers concerned with alleged biological information as in Jung's archetypes.
- Page, Michael, and Robert Ingpen, Encyclopedia of Things That Never Were. N.Y.: Viking, 1987. 240 pp. \$19.95. A lovely coffee-table book with interesting text, marvelous full-color illustrations and at a bargain price. Recommended to those interested in legends, folklore, and myth.
- Peat, F. David, Synchronicity: The Bridge Between Matter and Mind. New York: Bantam Books, 1987. 246 pp. \$8.95 paperback. A "New Age" consideration of Jungian synchronicity in terms of quantum theory speculations, especially those of David Bohm.
- Pickering, Andrew, Constructing Quarks: A Sociological History of Particle Physics. Chicago: University of Chicago Press, 1984. 468+xi pp. \$30.00 hardback, \$17.95 paperback. A very important work from the social constructionist position, seeking to demonstrate the social negotiation processes that are entailed in physics's construction rather than discovery of fundamental reality.
- Playfair, Guy Lyon, The Haunted Pub Guide. London: Harrap, 1985. 252 pp. 10.95 pounds ((UK). An illustrated guide and descriptive history (with some interesting commentaries by Playfair as well) to the many pubs throughout Great Britain where ghosts have been reported. Though most of the classic older cases are included, many tales Playfair dug up are quite recent. An excellent companion for either the armchair traveller or those who go to the U.K. and want to combine psychic "research" with folklore and beer.
- Playfair, Guy Lyon, If This Be Magic. London: Jonathan Cape, 1985. 284 pp. 9.95 pounds (UK). A wide ranging popular (nonscientific but intellectually stimulating) work on many facets of psychical research and the mind's capabilities for healing. Despite the reasonable tone throughout, the book is unlikely to convince serious skeptics; nonetheless, it is full of most interesting episodes and out-of-the-way information and references that should make the book valuable even to those not sharing all of the

the author's conclusions.

Pozos, Randolph Rafael, The Face On Mars: Evidence for a Lost Civilization? Chicago: Chicago Review Press, 1986. 155+vi pp. \$12.95 paperback. An excellent survey of the issues and problems presented by the 1976 Viking photograph of Mars which showed what to many appears to be "a large carving of a humanoid face lying on the ground and staring into space." Anthropologist Pozos presents a well balanced appraisal and history of the controversy, including the ad hominem attacks by astronomer critics of those seeking further objective appraisal of the evidence. After reading this book and considering the significance of the questions it raises, I am inclined to agree with David C. Webb's concluding statement in the book's Foreword: "Should the results of the Independent Mars Investigation Group eventually prove that intelligent life in a form similar to our own once existed on Mars, it would most surely indicate that just as war is too important to be left to the generals, so science is too important to be left only to the scientists!" A very interesting case study, whatever the eventual outcome.

Premack, David, Gavagai! Or the Future History of the Animal Language Controversy. Cambridge, Mass.: Bradford Books/MIT Press, 1986. 164+ix pp. \$12.50. An excellent brief review of much (but by no means all) of the controversy surrounding animals and their acquisition of language. Premack argues that humans are far more than apes plus language, anyway, and that too much discussion has centered on communication as the alleged sole human specialization.

Rae, Alastair I.M., Quantum Physics: Illusion or Reality. New York: Cambridge University Press, 1986. 123+x pp. \$9.95 paperback. An extremely lucid general introduction to the philosophical foundations and conceptual problems involved with the theory of quantum physics. In addition to discussions of the role of consciousness and the many-worlds hypothesis in relation to the measurement problem, there is an interesting critical discussion of purported quantum theory explanations of paranormal phenomena should be of special interest to ZS readers.

Ragon, Michel (translated by Alan Sheridan), The Space of Death: A Study of Funerary Architecture, Decoration and Urbanism. Charlottesville: University Press of Virginia, 1983. 328 pp. \$24.95. Truly an encyclopedic work on the world of the dead and funeral customs and beliefs. Fascinating reading and full of sociological insights into cultural variations and patterns. Places our views on survival and after-life into a broader and meaningful perspective.

Reid, Janice, Sorcerers and Healing Spirits: Continuity and Change in an Aboriginal Medical System. Canberra: Australian National University Press, 1986. 183+xxv pp. No price indicated, paperback. An ethnographic study of the Yolngu (Murngin) people in Arnhem Land and their medical theories about the relationships between social processes, supernatural power and human suffering.

Roberts, Jane (with introductory essays and notes by Robert F. Butts), Dreams, "Evolution," and Value Fulfillment, Volume I. New York: Prentice-Hall, 1986. 288 pp. \$15.95. The first of what will probably be several posthumously published volumes, edited by Roberts' husband. This volume in-

cludes not only material "dictated" by Seth, the alleged non-physical (and extraterrestrial) energy personality who spoke through Roberts in her many earlier volumes, but also tells the of the tragedy and tribulations of Roberts' illness which resulted in her death in 1984. Whatever one may think of Roberts'/Seth's optimistic and holistic philosophy, her books represent a remarkable case study in automatic writing, perhaps paralleling that of Patience Worth in its importance.

Rosenberg, Alexander, The Structure of Biological Science. New York: Cambridge University Press, 1985. 281+xi pp. \$37.50 hardcover; \$12.95 paperback. A general introduction to an integrated post-positivistic philosophy of biological science. An impressive work in its sophistication both philosophical and biological. Particularly valuable for its discussions of teleological and evolutionary issues.

Rosenthal, Robert, Meta-Analytic Procedures for Social Research. Beverly Hills, Cal.: Sage Publications, 1984. 149 pp. \$17.95 hardcover, \$8.95 paperback. An important introduction, requiring only a background of algebra, extending the work of Gene V. Glass and others on combining and comparing research results. Meta-analysis is an attempt to deal with problems of poor cumulation and small effects, problems common in anomaly studies, especially parapsychology. Recommended.

Rouget, Gilbert (translated and revised in collaboration with the author by Brunhilde Biebuyk), Music and Trance: A Theory of the Relations between Music and Possession. Chicago: University of Chicago Press, 1986. 397+xix pp. \$19.95 paperback. An important study in which the author develops a taxonomy of trance states and meticulously examines these in relation to various aspects of the music (the performers, instruments, ties to dance, etc.). He rejects past neurological and mental health explanations and argues that the relations between music and trance depend upon their interaction within the system of meanings in the particular culture.

Rudwick, Martin J.S., The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists. Chicago: University of Chicago Press, 1985. 494+xxxiii pp. \$39.95. An important case study in the history and sociology of science showing the resolution of a major scientific controversy in geology including such matters as suppression of and premature publications. This is a landmark case study with special importance for the issues of contextualism and role of social negotiation in science. The author's conclusion is that the cumulative empirical evidence did not determine the research results in any unambiguous way, as naive realists might claim, but it also did not result a purely social contest on the agnostic field as constructivists might contend; instead, it had a differentiating and constraining effect on the shape of the eventual picture of natural reality for which consensus emerged. Recommended.

Ryan, Alan, editor, Vampires: Two Centuries of Great Vampire Stories. Garden City, N.Y.: Doubleday/Dolphin, 1987. 621+xvi pp. \$15.95. An excellent introductory collection of vampire fiction, with well done introductions, chronologically arranged from 1816 to 1984, plus two brief indexes listing selected vampire novels and films.

Santilli, Ruggero Maria, Il Grande Grido: Ethical Probe on Einstein's Followers in the U.S.A. -- An Insider's View. Newtonville, Mass.: Alpha Publishing (897 Washington St.; Box 82; 02160-0082). 138 pp. \$19.50 paperback. A remarkable book by an Italian physicist, once associated with leading U.S. universities and currently head of the privately-funded Institute for Basic Research, alleging a conspiracy among academic and government physicists to block critical or generalizing work on Einstein's special relativity theory in which Santilli alleges they have vested "immense ethnic, financial and academic interests." Fascinating but unconvincing, the book may produce some interesting responses. More likely, it will be publicly ignored, and Santilli's supporters will see that as further corroboration of his conspiracy scenario.

Savage-Rumbaugh, E. Sue, Ape Language: From Conditioned Response to Symbol. New York: Columbia University Press, 1986. 433+xxviii pp. \$40.00. A very important survey of the history and controversy surrounding ape language research and the recent experimental work by Savage-Rumbaugh (one of the leading advocates of the apes-can-symbolize view) with the male chimpanzees Sherman and Austin and a pygmy chimpanzee, Kanzi.

Schwartz, Stephan A., The Alexandria Project. New York: Delacorte Press/Eleanor Friede, 1983. 274+x pp. \$18.95. A popularly written description of the Mobius Group's adventure in psychic archaeology. Impossible to evaluate its scientific value without examination of the technical reports and information from normal archaeologist-critics not in this book, but the book presents a readable tale of an interesting and promising approach to the area and one which needs responsible critical scrutiny it so far has not obtained.

Scott, Beth, and Michael Norman, Haunted Heartland. New York: Warner Books, 1986. 487+xiv pp. \$3.95 paperback. An excellent popular rather than academically (boringly?) presented collection of "authentic" ghost folktales from the Midwest states gathered from a wide range of sources.

Steiger, Brad, Demon Lovers. New Brunswick, N.J.: Inner Light Publications, 1986. 200 pp. \$9.95 paperback. An unscholarly and mildly sensationalistic potboiler for the occult marketplace by a prominent pseudo-parapsychologist author. Not without interest but impossible to ascertain what is remotely reliable among the many anecdotes.

Stevens, Austin, editor, Mysterious New England. Dublin, N.H.: Yankee Publishing Inc., 1985 (Third edition). 319 pp. \$11.95 paperback. A selection of 50 enjoyable and purportedly true gothic tales set in rural New England, reprinted from Yankee Magazine.

Stevenson, Ian, Unlearned Language: New Studies in Xenoglossy. Charlottesville: University Press of Virginia, 1984. 223 pp. \$17.50. Examination of reported instances, primarily through analysis of two case studies, of persons speaking foreign languages which they apparently had no opportunity to learn. Since Stevenson believes languages must be learned, such authentic cases may be viewed as evidence suggestive of learning in a past life, thus fitting into Dr. Stevenson's past writings on reincarnation. An important work on fascinating materials, but essentially proposing an explanation perhaps more extraordinary than the extraordinary phenomenon itself.

Stove, David, Popper and After: Four Modern Irrationalists. New York: Pergamon, 1982. 116+viii pp. \$9.50 paperback. A consideration of Karl Popper, Thomas Kuhn, Imre Lakatos, and Paul Feyerabend as "irrationalists" in that Stove reads them as entirely denying cumulative progress in science, a position which I think grossly oversimplifies and thereby misrepresents their positions. Still, Stove makes some good points and the defenders' rebuttals should prove interesting and clarifying.

Sullivan, Jack, editor, The Penguin Encyclopedia of Horror and the Supernatural. New York: Viking Penguin, Inc., 1986. 482+xxx pp. \$29.95. An excellent compendium on horror fiction, its subject matter and personalities, by over fifty commentators. Particularly valuable for its discussions of films, many of the essays on such subjects as poltergeists and vampires should be of interest to some ZS readers.

Szamosi, Geza, The Twin Dimensions: Inventing Space and Time. New York: McGraw Hill, 1986. 289+viii pp. \$15.95. A physicist's broadly interdisciplinary examination of our changing constructions of space and time (not only in physics but in music and the arts) from early mythology to today's picture of an inflationary universe.

Talbot, Michael, Beyond the Quantum. New York: Macmillan, 1987. 240+xiii pp. \$18.95. An attempt to tie in the new physics with issues of God, reality, the paranormal, and consciousness. Highly speculative but anchored in the serious physics literature and should prove provocative to ZS readers.

Tansley, David, The Raiment of Light: A Study of the Human Aura. New York: Routledge & Kegan Paul, 1984. 198+x pp. \$9.95 paperback. An interesting but mostly uncritical (and largely historical) review of the literature on all aspects of the alleged human aura. Useful for its coverage and attempt to integrate a large related literature, but not at all scientifically convincing (nor does it claim to be so).

Tart, Charles T., Waking UP: Overcoming the Obstacles to Human Potential. Boston: Shambala New Science Library, 1986. 323+xvi pp. \$17.95. A mixture of human potential psychology, Aikido, Buddhism and especially Gurdjieff, claiming a road to awakening from the sleep-like consciousness of normal awareness.

Tausig, Michael, Shamanism, Colonialism, and the Wild Man: A Study in Terror and Healing. Chicago: University of Chicago Press, 1987. 518+xx pp. \$29.95. A remarkable anthropological formulation of the relations between magic and colonialism, imagemaking and power incorporating ethnography of shamanic healing in Columbia and a novel studied disorderliness of style.

Terry, Maury, The Ultimate Evil: An Investigation of America's Most Dangerous Satanic Cult. Garden City, N.Y.: Doubleday/Dolphin, 1987. 512+xiii pp. \$17.95. Essentially a conspiracy tale alleging connections between David Berkowitz ("Son of Sam"), Charles Manson, the Process Church, and various "satanic" slayings around the U.S. Many large leaps in logic and much contestable evidence, but some fascinating stuff. Worse, it could be true. Meanwhile, its more fuel for the modern witch-hunters.

Urban-Lurain, Mark, Astrology as Science: A Statistical Approach. Tempe, Arizona: American Federation of Astrologers (P.O. Box 22040; 6535 South

- Rural Road; 85252), 1984. 77+ix pp. No price indicated, paperback. Originally a 1981 master's thesis in the Multidisciplinary Program in the College of Social Science at Michigan State University, then entitled "A Multidimensional Model for Evaluating Astronomical Concomitancies of Human Behavior." A study using birth data for 53 Alcoholics Anonymous members controlled against a sample of the general Michigan population. 9.4% of the variables were discriminatory at the .05 level, and the author concludes that such multidimensional statistical models (which are also more similar to the actual processes used by astrologers in interpreting horoscopes) have greater discriminatory power than do the univariate models usually used to examine astrological claims. An important study, especially given its publication/endorsement by the AFS.
- Van Helden, Albert, Measuring the Universe: Cosmic Dimensions from Aristarchus to Halley. Chicago: University of Chicago Press, 1984. 203+vi pp. \$30.00. A fascinating work in the history of astronomy and on the impact of the telescope in overthrowing the Ptolemaic perspective.
- Vaughn, Francis, and Roger Walsh, editors, Accept This Gift: Selections from A Course in Miracles. Los Angeles: Jeremy P. Tarcher, 1983. 107 pp. \$9.95. A collection of "inspirational" statements culled from the 3-volume A Course in Miracles published in 1976 as "received" via an "inner voice" by psychologist Helen Schucman and "recorded" with fellow psychologist William Thetford. Many pithy aphorisms like "All anger is nothing more than an attempt to make someone feel guilty" and "What is there to be saved from except illusions?" which upon serious reflection reveal the path to truly be "spongy tissue" and intellectually shallow --whatever their psychological value for the troubled. Mostly escapist with the emphasis on retreat into inner space. Therapeutic for some, perhaps, but I found it insipid. Given its incredible popularity, someone should do a content analysis of the full original work. That might be quite revealing about the mentality of the readership.
- Vickers, Brian, editor, Occult & Scientific Mentalities in the Renaissance. New York: Cambridge University Press, 1984. 408+xiv pp. \$39.50. An excellent collection of symposium essays dealing with the occult and scientific mentalities and their interactions in the works of scientists like Newton, Kepler, Bacon, and Merenne, and occultists like Dee, Fludd, and Cardano. Demonstrates the oversimplification of the standard magic-to-science perspective. Highly recommended.
- Vieira, Waldo, Projeciologia: Panorama das Experiencias de Consciencia Fora do Corpo Humano. Rio de Janeiro, Brasil: Privately published by the author, 1986. 866+xxvii pp. Limited (5000) complementary copies distributed by the Centro de Consciencia Continua.
- Villoldo, Alberto, and Stanley Krippner, Healing States. New York: Simon and Schuster, 1987. 208+xvi pp. \$8.95 paperback. A cross-cultural look at the mind's ability to heal, comparing Western medicine with shamanic and spiritual healing in North and South America. Though dealing with medically highly controversial materials, this is a reasonable presentation.
- Weil, Andrew, Health and Healing: Understanding Conventional and Alternative Medicine. Boston: Houghton Mifflin, 1985. 296 pp. \$7.95 paperback. An important call for allopathic medicine which critically surveys conventional medicine and sympathetically considers alternative approaches. Though I have much sympathy with Weil's criticisms of orthodox medical practice, he shows an unfamiliarity with much of the responsible criticism of the paranormal literature, e.g., in his section on firewalking. Still, the book is generally well balanced and a needed antidote to many current practices.
- Wells, Garyuth L., and Elizabeth F. Loftus, editors, Eyewitness Testimony: Psychological Perspective. New York: Cambridge University Press, 1984. 374+10 pp. \$39.50. A fine collection of readings dealing with the experimental literature on the problems of eyewitness testimony, especially as evidence in criminal cases. Of special relevance to those concerned with witness reports on anomalies. Recommended.
- Wilson, David B., editor, Did the Devil Make Darwin Do It? Modern Perspectives on the Creation-Evolution Controversy. Ames, Iowa: Iowa State University Press, 1983. 241-xxii pp. \$13.95 paperback. An excellent collection of multi-disciplinary, antirecreationism essays, featuring a section on the battles of creationism's efforts to enter public education.
- Wallace, Amy, The Prodigy. New York: E.P. Dutton, 1986. 297+xi pp. \$18.95. An excellent biography of William James Sidis, perhaps the greatest child prodigy of record (his I.Q. has been estimated as being 50 to 100 points higher than Einstein's). His extraordinary early development and later enigmatic "decline" remains controversial, but Wallace presents a plausible scenario of explanation. Those concerned with the paranormal might usefully examine such cases of incredible but "merely" abnormal psychology to see that well validated wonders also appear the rare end of the normal spectrum.
- Watkins, Peter, Story of W and Z. New York: Cambridge University Press, 1986. 240+xii pp. \$44.50 hardbound; \$13.95 paperback. A first-hand and non-technical account of the discovery of the W and Z boson in 1983 at Geneva's CERN laboratory, for which Carlo Rubbia and Simon van der Meer were awarded the Nobel Prize in Physics in 1984. Of special interest to historians of science, ZS readers should find some of the final chapter which deals with the future of physics of particular interest.
- Weber, Renee, Dialogues with Scientists and Sages: The Search for Unity. New York: Routledge and Kegan Paul, 1986. 256 xvi pp. \$14.95 paperback. A series of interviews/dialogues with scientists (e.g. Rupert Sheldrake, David Bohm, Stephen Hawking, and Ilya Prigogine) and mystics (e.g., Krishnamurti, the Dalai Lama, Father Bede Griffiths, and Lama Govinda), six of which earlier appeared in Re-Vision Journal.
- Whincup, Greg, Rediscovering the I Ching. Garden City, N.Y.: Doubleday, 1986. 238+xv pp. \$18.95. A new translation and scholarly reinterpretation of the Book of Changes which argues that the original was far more pragmatic and less abstrusely meditative than has been commonly believed. An important new work on the history of Chinese divination systems.
- White, John, editor, What Is Enlightenment? Exploring the Goal of the Spiritual Path. Los Angeles, Cal.: J.P. Tarcher, 1985. 232+xx pp. \$9.95 paperback. A sequel/update to White's 1971 The Highest State of Consciousness containing 33 readings from leaders in the quest of self-transcendence.

Wilson, Colin, Afterlife. Garden City, N.Y.: Doubleday Dolphin, 1987. 269 pp. \$16.95. In some ways a sequel to his Mysteries volume, here Wilson examines a very wide range of materials dealing with the cases for (and to a much lesser and quite inadequate extent) against survival. As is the case with Wilson's other writings on psychical research, he seems unfamiliar with (at least does not cite or give reasons for dismissing) the contrary literature (in this case not only from the "standard" anti-spiritualist critics like Trevor Hall and Milbourne Christopher but the substantial serious philosophical literature dealing with mind-body issues). This very one-sided approach not surprisingly concludes in favor of survival. Nonetheless, the wide ranging coverage of the positive literature makes it a good introduction to the proponents' arguments.

Wilson, Robert Anton, The New Inquisition: Irrational Rationalism and the Citadel of Science. Phoenix, Ariz.: Falcon Press, 1986. 240+iii pp. \$9.95 paperback. A remarkable romp through the halls of Scientism and a plea for Wilson's brand of "creative agnosticism." Should be of great interest to many ZS readers, some of whom (especially doctrinaire supporters of CSICOP will be infuriated while others will be enlightened (and a few will be both). Though I don't share Wilson's epistemological views, reading him is a great breath of fresh air (and not just Breathairians will find food for thought here). His writing is provocative, funny and original, and he has done his homework. In short: great fun. I only wish the book had an index.

Wise, David, The Samarkand Dimension. Garden City, NY: Doubleday, 1987. 303 pp. \$16.95. An espionage novel dealing with Soviet and U.S. government psi research. Despite some problems for me with the storyline, the author has done his homework, and many ZS readers will enjoy the details and the plot twist.

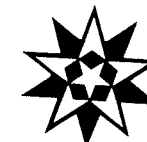
Wolman, Benjamin B., and Montague Ullman, editors, Handbook of States of Consciousness. New York: Van Nostrand Reinhold Co., 1986. 672+x pp. \$54.95. An excellent collection of 20 papers comprehensively dealing with consciousness studies covering theory, manifestations and applications. Much of interest for ZS readers ranging from psi research to lucid dreaming to multiple personality. Recommended.

Zaleski, Carol, Otherworld Journeys: Accounts of Near-Death Experiences in Medieval and Modern Times. New York: Oxford University Press, 1987. 275+ix pp. \$18.95. An important survey by a Lecturer on the Study of Religion at Harvard University, adding historical perspective to current scientific and religious dialogues on the meaning of near-death narratives. Particularly important for it delineation of the differences and similarities between such reports in medieval Christendom and modern secular America.

Zolar, Zolar's Compendium of Occult Theories and Practices. New York: Prentice-Hall Press, 1986. 391+vi pp. \$9.95 paperback. The epitome of popular culture occultism which can not even come up to the level of pseudoscience since it makes no pretense of being scientific. However, even serious occultists would view this stuff as pop-occultism and would reject it. Presumably geared to the mentality of those who read the tabloids not for entertainment but for hard news.



# CSAR



## Statement of Goals

The Center for Scientific Anomalies Research (CSAR) is a private center which brings together scholars and researchers concerned with furthering responsible scientific inquiry into and evaluation of claims of anomalies and the paranormal. The Center will:

- \* Advance the interdisciplinary scientific study of alleged and verified anomalies.
- \* Act as a clearinghouse for scientific anomaly research.
- \* Create an international and public network of experts on anomaly research through publication of the CSAR Directory of Consultants.
- \* Publish a journal (Zetetic Scholar), a newsletter (The Csar Bulletin), research reports, and bibliographies.
- \* Promote dissemination of reliable and objective information about scientific anomaly research.
- \* Sponsor conferences, lectures, and symposia related to anomaly research.
- \* Promote improved communication between critics and proponents of scientific anomaly research.

The primary focus of the Center will be on the study and evaluation of bodies of anomalous observations rather than upon esoteric theories seeking to explain already known phenomena. The orientation of the Center is exclusively scientific, places the burden of proof on the claimant, and recognizes the need for a degree of proof commensurate with the extraordinary character of the phenomenon claimed. But the Center also wishes to promote open and fair-minded inquiry that will be constructively skeptical. We recognize that scientific anomalies, where valid, may be instruments and driving forces for reconceptualization and growth in scientific theory. Critically and constructively approached, legitimate anomalies should be welcomed by science rather than perceived as ill-fitting nuisances. History clearly demonstrates that tomorrow's science is likely to contain surprises, and tomorrow's theories are likely to explain some of what are today viewed as controversial anomalies. Also, tomorrow's explanatory theories may be in areas of science not now perceived as relevant to the anomalies being considered. Thus, "anomalistics" must necessarily be an interdisciplinary endeavor.

CSAR was founded in 1981 by its Director, Prof. Marcello Truzzi. The Associate Director of CSAR is Prof. Ron Westrum. Both are sociologists at Eastern Michigan University. CSAR is sponsored by a group of distinguished scientists and scholars who have agreed to act as Senior Consultants.

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CSAR is a private Center whose policies and governance are under the control of its governing board. Members and Consultants thus do not control CSAR, but their suggestions and criticisms are always welcome by the governing board. There are a variety of associations individuals may have with CSAR. These include:

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